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ART. XXII.—*On the Amount of the Atmospheric Absorption*; by S. P. LANGLEY.

[From a communication made to the Nat. Academy of Sciences, in April, 1884.]

THE earth is surrounded by an absorbing atmosphere, and we never see the sun or the stars except through it.

When we wish to know what the absolute brightness of the sun or of a star is, we must then first ask what the degree and kind of this absorption has been, and must add to the directly observed quantities of light, the amount that the atmosphere has taken away. Accordingly, every one engaged in such researches must determine in explicit or implicit terms for himself, or take on trust through another, the amount of the absorption, which there is great unanimity in fixing at about 20 per cent of the whole (at the sea level.) Thus the earliest observations in the last century give the light absorption as 19 per cent. The very elaborate ones by Seidel of Munich give 21 per cent, those by Pritchard at Oxford, 21 per cent; the most recent by Mueller at Potsdam, 17 per cent; while the observations by Pouillet on the sun's heat give 18 to 24 per cent; and almost all of a great number which could be cited, whether on light or heat, give about 20 per cent. It has indeed been recognized of late years that the "light" rays are on the whole more absorbable than those of "heat," and that, in particular, blue light is much more so, but the difference between the mean coefficients of "light" and "heat" as found by the usual methods is so small that we may here continue to speak of this "light" absorption of 20 per cent as closely applicable (in common estimation) to heat also. Thus, the very careful series of Ericsson on the sun's

heat gives about 21 per cent. In fact, many thousands of observations by scores of observers, during the past and present century, seem to have determined this proportion with all the exactness of which it is capable.

I have arrived at a result so wholly different, that, in the face of such authority, I almost hesitate to announce it; for I have been forced to the conclusion, that all these determinations are in error; and not in some small degree, but by a quantity probably at least equal to the total amount in question.

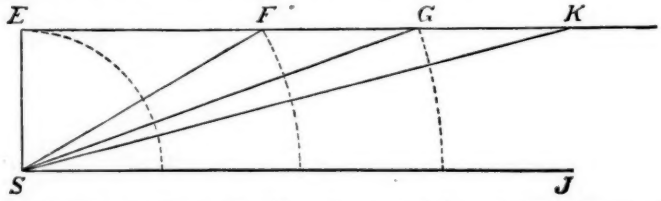
I hasten to say, that I do not dispute the general accuracy of the numerous skillful investigators of known repute who have made these determinations, but that I dissent from the method in almost universal use for reducing them, for since it is certain that none of these observers have been outside the atmosphere to see what the radiation really is; all of them, however skilled, must depend on inference to determine what it would be, if they could thus observe it. It is certain that nearly all have used a formula of which it seems capable of absolute demonstration that it is not only erroneous, but that its error always lies in one direction, so as to invariably make the calculated absorption too small, and it may be further shown with an evidence which seems little less than demonstrative, that the numerical value of the error is very large in relation to the quantities involved.

I have been led however, not by theoretical considerations alone, but by experimental investigation, (during the course of which I have observed both near the sea-level and at great altitudes), to the conclusion that the laws under which solar and stellar light and heat are absorbed by the atmosphere, are so complex that their complete comprehension is still beyond our power, but that we may at least now improve our present ways of studying them, for investigators have, however skillful in their observations, commonly ignored the complexity of the problem of their reduction, and, assuming that it is as simple as we could wish it to be, have proceeded to compute the result by such a formula as it would be most convenient for us, if nature would follow. Thus, owing to a natural tendency to accept as sufficient any scientific dogma which has respectable sponsors and which saves labor, the simple rule, established over a century ago by Bouguer, and consecrated by the use of Herschel, and Pouillet, to whom it embodied all the knowledge of their time, is commonly used by us to-day; though we may easily be convinced that it has ceased to express the facts known to ours.

To justify this language, let us consider what the problem appears to be, at first glance, and what the first suggestion is for solving it. If a beam of sunlight enters through a crevice in a dark room, the light is partly interrupted by the particles of dust or mist in the air, the apartment is visibly illuminated by

the light laterally reflected or diffused from them, and the direct beam, having lost something by this process, is not so bright after it has crossed the room, as before. In common language, the direct light, to an observer in the path of the beam, has been partly "absorbed," and the problem is, to determine in what degree. If a certain portion of the light (suppose one-fifth) was thus scattered, the beam after it crossed the room would be but four-fifths as bright as when it entered it; and, if we were to trace the now diminished beam through a second apartment altogether like the other, it seems at first, reasonable to suppose that the same proportion (i. e. four-fifths of the remainder) would be transmitted there also, and that the light would be the same kind of light as before, and only diminished in amount (in the proportion $\frac{4}{5} \times \frac{4}{5}$.) The assumption originally made by Bouguer* and followed by Herschel and Pouillet, was that it was in this manner that the solar heat was absorbed by our atmosphere, and that by assuming such a simple progression the original heat could be calculated. (The relatively minute expenditure of energy in the actual warming of the air is of course to be included.)

Let us (to repeat Bouguer's reasoning) divide in imagination any homogeneous absorbing medium, into successive strata of identical thickness and chemical constitution.



Let A be a source of radiant heat or light, whose intensity is reduced by passage through the first stratum to (let us suppose) a fraction of the original represented by p, so that what was A becomes Ap. Then, since the second stratum is identical with the first in constitution and amount, and must (it is assumed), have an identical effect, it will, on Bouguer's hypothesis, transmit p of what enters it, and Ap^2 will emerge from the second, and so on, the fraction p transmitted by the unit of thickness (the "coefficient of transmission") being evidently the common ratio of a geometrical progression, so that if the original heat be A, the amount of heat after passing through e strata will be Ap^e and the amount transmitted at any point will be proportional to the ordinate of a logarithmic curve.

To apply this to the estimate of the heat outside the atmosphere (i. e. before absorption), let SJ be a small portion of the

* Bouguer, "Traite de la lumière," Paris, 1760.

earth's surface, and EK the upper surface of the atmosphere, which is here supposed to be of uniform density and constitution. (The effects of the actually unequal density of successive strata, can, it is assumed, be calculated and allowed for.) Let S be the observer's station, then ES would be the direction of a ray where the sun is in the zenith, and, to fix our ideas, let FS=2 ES; GS=3 ES; KS=4 ES, etc. The original heat A would become Ap after passing through one stratum (ES); and, according to what has been assumed, it would become (when the sun's zenith distance became ESF) Ap^2 after absorption by the two strata between F and S, Ap^3 after absorption by the three strata between G and S, etc. A, the original heat, and p the coefficient of transmission, are unknown: but if we make an observation of the heat actually reaching S along FS (let us call this heat m) and again later in the day along GS (calling this second observed quantity n) we have in the particular case supposed

$$Ap^2=m \qquad Ap^3=n$$

whence A and p both become known. Designating the number of strata by e and the observed values (m or n) by t, we have then $Ap^e=t$; the exponential formula of Pouillet and later investigators. Its fundamental (and erroneous) assumption is, that the coefficient of transmission (p) is a constant.

It is no doubt true that a very sensible portion of the solar rays is scattered by an analogous process in our atmosphere; but we have in our present knowledge to consider that by whichever of its effects we note it, this radiation is not simple but complex. Thus, we must remember that heat, like light, is of different kinds. To use Melloni's illustration, radiant heat would to any eye that could '*see*' it, appear to be of totally different colors, and hence, it may be added, we ought no more to attribute to it a single rate of absorption with regard to any absorbing medium, than to assume that a blue and a red ray would pass through a red glass with equal facility. The statement of this fact, may perhaps seem superfluous to the reader, for it has long been in one sense well known. But in another and most important sense it is not well known, and it cannot be superfluous to recall its bearing on our present research, since it is the neglect to follow it into its consequences which has led to the error in question. Since then the solar energy, whether regarded in whole as "heat" or "chemical action," or in part as "light," is the sum of an infinite number of radiations, these may be conceivably influenced in an infinite diversity of ways by the different atmospheric constituents. In fact the larger particles rather reflect than diffuse the heat or light, and hence treat all wave-lengths nearly alike, or diminish the direct radiation by a nearly general absorption: minuter ones begin to

act selectively, or on the whole more at one end of the spectrum than the other: smaller particles, whether of dust or faintest mist, and smaller still, form a probably continuous sequence of more and more selective action, down almost to the actual molecule, whose vibration is felt in the purely selective absorption of some single ray. The effect of the action of the grosser dust particles then, is to produce a general and comparatively indifferent "absorption" of all rays, after which the spectrum would everywhere seem equally less bright and less hot. The effect of the molecular absorption, is to fill the spectrum with evidences of the selective action in the form of dark telluric lines, taking out some kinds of light and heat, and not others, so that after absorption what remains is not only less in amount but *altered in kind.* Between these two extreme examples of absorption, we repeat, an unlimited number of others must exist; but we shall need here for simplicity to first treat the whole as coming under one or the other of these two types, a procedure already more accurate than the primitive one followed by Bouguer, Herschel and Pouillet, but which we recognize as still but a convention, which is imposed on us provisionally by the actual complexity of nature.

It will be seen now more clearly, that the whole primitive process followed by Pouillet is an assumption, for it is simply taken for granted that the same proportion will be absorbed by one like stratum as by another. On actually trying the experiment, however, with media in the laboratory, Melloni long since observed that like proportions were *not* absorbed by like strata; and the reason was found in the fact just noted, that radiant energy is not a single emanation, but the sum of an infinity of diverse ones, each with its own separate rate of absorption. It follows that the coefficient of transmission is truly constant only in the case of the absolutely homogeneous ray, which the ordinary photometer or thermometer cannot in the least discriminate; and which the finest linear thermopile or bolometer can but approximately discern, and hence, that the original light of the star, or heat of the sun, and the amount absorbed, can at best only be found approximately. However familiar this principle may be in some departments of physics, astronomers and meteorologists have been slow in making this application to the present case, but have continued to deduce the brightness of the stars, or the Solar Constant, from observations in which the radiation is either treated as absolutely homogeneous or in which its non-homogeneity is scarcely recognized as a factor of importance.*

* Exceptions to this remark are however to be made in favor of the very early work of Principal Forbes (Royal Society's Philosophical Transactions, May, 1842), and of the more recent labor of M. Crova (Academie des Sciences de Montpellier,

This neglect to make what seems so pertinent an application of Melloni's observation, even after it had been explained and extended (by Biot) will seem more explicable, when it is remembered that no direct means of measuring the absorption in even approximately homogeneous rays till very recently existed, and that a departure from the old formula, which ignores the difficulties, involves their recognition, and the devisal of new processes to meet them. Even if we, by the employment of such new processes, succeed in measuring the absorption in approximately homogeneous rays, the approximation is chiefly to homogeneity in wave-length, and not to uniformity of physical properties in consecutive wave-lengths, so that we are unable to represent the absorption as any continuous function of the latter. In other words, we may measure on separate narrow portions ($\Delta\lambda_1$, $\Delta\lambda_2$, etc.) of the spectrum and determine for each its apparent coefficient of transmission, (p_1, p_2, etc.) which is in each case some function of the wave-length, but we are not at liberty to write that the original energy of the heavenly body

$$A, = \int_{\lambda_n}^{\lambda_m} (\varphi\lambda)^{-\varepsilon} d\lambda$$

since our $\varphi\lambda$ is really discontinuous, a remark the import of which will become more apparent in the sequel. For the present at least, we are at liberty only to divide the spectrum into a finite number of parts and to sum the results.

I have already stated elsewhere* that in neglecting the fact that the absorption is really selective, we not only commit an error, but an error that always lies in one way, so that any determination of the absorption we make by the ordinary and erroneous formula never errs by being too great, but is, so far as it depends on this formula, always, and invariably, too small. The demonstration may be put in an extremely simple form, but I am not aware that it has been elsewhere given, though it was indicated in the proceedings just cited.

Let us first suppose the radiation of the heavenly body to be really composed before absorption of two portions, A and B.

1876). See also the excellent little treatise "Actinometrie" by M. Radau. The use of two coefficients is proposed in this, as it has been before, but does not seem to have been followed by others, who like M. Violle, have subsequently (Annales de Chimie et de Physique, 1879) employed but a single coefficient of transmission. Still more lately, however, the importance of the consideration on which the writer here insists has been remarked on by Messrs. Lecher and Pernter, and perhaps by others. The employment of the method of Forbes, especially as modified and extended by Crova, appears to be the best means at the command of the observer with the actinometer or photometer. This method, however, is unfortunately very limited in its practical application, owing to the insufficiency of data thus obtainable, and it still gives a necessarily too small result, though a larger one than Pouillet's.

*Comptes Rendus de l'Acad. des Sc., xcii, 701, March 21, 1881.

Let A have a special coefficient of transmission (a), and B another, special to itself (b). Then, if we assume (still for considerations of convenience only) that each of these portions, is, separately considered, homogeneous, we may write down the results in the form of two geometrical progressions, thus:

TABLE 1.

Original radiation.	Ratio.	Radiation received after absorption by one stratum.	By two strata.	By three strata.	By four strata, etc.
A	a	Aa	Aa^2	Aa^3	Aa^4
B	b	Bb	Bb^2	Bb^3	Bb^4
A+B		$Aa+Bb$ =(M)	Aa^2+Bb^2 =(N)	Aa^3+Bb^3 =(O)	Aa^4+Bb^4 =(P)

Then will

$$\frac{Aa+Bb}{A+B} < \frac{Aa^2+Bb^2}{Aa+Bb} < \frac{Aa^3+Bb^3}{Aa^2+Bb^2} < \frac{Aa^4+Bb^4}{Aa^3+Bb^3} < \text{etc.}$$

$$\text{and } \frac{Aa^2+Bb^2}{Aa+Bb} < \left(\frac{Aa^3+Bb^3}{Aa+Bb}\right)^{\frac{1}{2}} < \left(\frac{Aa^4+Bb^4}{Aa+Bb}\right)^{\frac{1}{3}} < \text{etc. . . .}$$

The fractions here are the coefficients of transmission, as deduced from observations at different zenith distances. They evidently differ, and (as will be shown) each is larger than the preceding.

In the above table $Aa+Bb$ is the sum of the two kinds of radiation as observed after absorption by one unit stratum (sec. $\zeta=1$) by the photometer, or actinometer; Aa^2+Bb^2 is the sum of the radiations observed after absorption by two strata (sec. $\zeta=2$) etc.; but we are here supposed to independently know the really dual constitution of the radiation, which the photometer or actinometer does not discern. According to the usual hypothesis, the coefficient of transmission, which is the quotient obtained by dividing the value after n absorptions by that after $n-1$ absorptions, or more generally that from the expression

$$\left(\frac{\text{Value after } n \text{ absorptions}}{\text{Value after } m \text{ absorptions}}\right)^{\frac{1}{n-m}}$$

is a constant. It is in fact not a constant, as we shall prove later; but we shall first show that, if we proceed upon the ordinary assumption, the value obtained for the original light of the star before absorption will in this case be too small. For, if we observe by a method which discriminates between the two radiations, we shall have, if we separately deduce the original lights from our observation of what remains after one and again after two absorptions, the true sum

$$A+B=\frac{(Aa)^2}{Aa^2}+\frac{(Bb)^2}{Bb^2}$$

while if we observe by the ordinary method, which makes no discrimination, we shall have the erroneous equation

$$A+B=\frac{(Aa+Bb)^2}{Aa^2+Bb^2}$$

which is algebraically less than the first, or correct value, for the expression

$$\frac{(Aa)^2}{Aa^2}+\frac{(Bb)^2}{Bb^2}>\frac{(Aa+Bb)^2}{Aa^2+Bb^2}$$

readily reduces to the known form

$$a^2+b^2>2ab.$$

Moreover since $a^2+b^2-2ab=(a-b)^2$, *the error increases with the difference between the coefficients.*

Now, in the general case, if we suppose the original radiation L to be composed before absorption, of any number of parts A_1, A_2, A_3, + . . . having respectively the coefficients of absorption a_1, a_2, a_3, + . . . the true value of L is given by a series of fractions which may be written in the form

$$L=\Sigma\frac{(Aa)^2}{Aa^2}=\Sigma A$$

whereas the value of the original energy by the customary formula would be

$$L_1=\frac{\Sigma\,(Aa)^2}{\Sigma\,Aa^2}$$

so that, all the quantities being positive, by a known theorem, $L>L_1$, and for the same values of A_1, A_2, A_3, ... this inequality is greater, the greater the difference in the values of the coefficients a_1, a_2, a_3 . . .

But this is stating in other words that the true values, found by observing separate coefficients of transmission, are *always* greater than those found when we do not distinguish between the radiations of which the light (or heat) of the star or sun is composed, and also that the amount by which the true values are greater, increases with the difference between the coefficients.

We have stated above that the usual hypothesis makes the coefficient of transmission a constant. It will be seen from the above table, however, that it varies from one stratum to the next; that it is least when obtained by observations near the zenith; and that it *increases progressively as we approach the horizon.* For since a and b are each less than unity, each of the sums $Aa+Bb$, etc., in the above table is less than the preceding. It is also evident that their rate of diminution

decreases as we approach the horizon, since

$$Aa^2 - Aa^3 > Aa^3 - Aa^4 \qquad Bb^2 - Bb^3 > Bb^3 - Bb^4.$$

Hence

$$(Aa^2 + Bb^2) - (Aa^3 - Bb^3) > (Aa^3 + Bb^3) - (Aa^4 + Bb^4).$$

Consequently the difference between the numerators of two successive ratios, such as

$$\frac{Aa^3 + Bb^3}{Aa^2 + Bb^2} < \frac{Aa^4 + Bb^4}{Aa^3 + Bb^3}$$

is less than that of their denominators. In other words, although both numerator and denominator decrease in successive ratios, the ratios themselves increase progressively, and a similar demonstration applies to the form

$$\frac{Aa^2 + Bb^2}{Aa + Bb} < \left(\frac{Aa^n + Bb^n}{Aa + Bb}\right)^{\frac{1}{n-1}}.$$

But these ratios are the coefficients of transmission in question.

Again, a simple inspection of the form of the expression,

$$Aa^2 - Aa^3 > Aa^3 - Aa^4 \qquad Bb^2 - Bb^3 > Bb^3 - Bb^4$$

shows that what is there demonstrated for two numbers and two coefficients A, *a*, and B, *b*, is true for any number, even infinite, which is the case we deal with in actual observation.

It is then universally true that when the numbers are positive, and *a*, *b*, *c*, *d*, ... proper fractions

$$\frac{Aa^{n+1} + Bb^{n+1} + Cc^{n+1} + Dd^{n+1} + \ldots\ldots}{Aa^n + Bb^n + Cc^n + Dd^n + \ldots\ldots} <$$

$$< \frac{Aa^{n+2} + Bb^{n+2} + Cc^{n+2} + Dd^{n+2} + \ldots\ldots}{Aa^{n+1} + Bb^{n+1} + Cc^{n+1} + Dd^{n+1} + \ldots\ldots}$$

and hence universally true, that when the separate coefficients of transmission are positive and less than unity (as is the case in Nature), the general coefficient of transmission in the customary exponential formula is,

(1) *never a constant*, and (as determined from the customary formula),

(2) *always too large*,

(3) *always larger and larger as we approach the horizon.*

(4) The original light or heat of the heavenly body as found by the photometric and actinometric processes, and the formulæ, in universal use, *is always too small*, a conclusion which we have just reached by another method.

The above demonstration does not tell us in how great a degree this coefficient is too large, and for aught we have here yet shown, the error may be practically negligible.

Since the method ordinarily employed demonstrably gives too small results, the burden of proof might seem to rest on

those who still employ it, who might now with propriety be asked to show that the continued use of methods and formulæ, certainly in some degree inaccurate, does *not* lead to an error at least as great as the total absorption in question. This has never been done. There is a common assumption that if there were any considerable error, its results would become apparent in such numerous observations as have been made all over the world in stellar photometry and solar actinometry during this century, since in these observations of stellar magnitudes, for instance, two stars whose relative magnitudes are positively known, give results closely agreeing with the ordinary formula when one is near the zenith and the other near the horizon. At first this looks almost like evidence that there can be no great error in the determination of absolute magnitudes by the ordinary formula, and yet this apparent proof is demonstrably a fallacy It is certainly a specious one, but it is absolutely demonstrable that the error *might* be enormous—that the actual absorption might be, for instance, 50 per cent instead of 20, without this gross discrepancy being detected by our present modes of observation. As the present methods are known to give, as I have just said, values in substantial agreement (within three or four per cent) from independent observations made at very different altitudes of the sun or star, the proof of this last statement may well be demanded, and I proceed to give it.

(It is admitted that the original energy is divisible into an unlimited number of rays, each characterized by its own coefficient of transmission, and that these coefficients have values varying from zero to unity.) Let this original energy of the heavenly body be X, and let us denote as in table 1 the observed energy after one absorption by M, after two, by N, after three, by O, etc.

The assumption of those who admit the existence of an error but maintain that it is negligible, is that since we do know that $O:N=N:M$ very nearly, *therefore* $N:M=M:X$ very nearly, or in other words that if observation proves that $M=\frac{N^2}{O}$ within three or four per cent, we are entitled to assume that there is only a like small error in writing $X=\frac{M^2}{N}$.

We can make the fallacy of the preceding assumption most obvious by first considering a particular case. Let the original energy be divided into any number of parts A, B, C, etc., each with its own coefficient a, b, c, etc., so that

$$\begin{array}{llll} Aa+Bb+Cc+Dd+++Kk+Ll & \text{etc.}=M \\ Aa^2+Bb^2+Cc^2+Dd^2+++Kk^2+Ll^2 & \text{etc.}=N \\ Aa^3+Bb+Cc^3+Dd^3+++Kk^3+Ll^3 & \text{etc.}=O \\ \text{etc.,} \qquad\qquad \text{etc.,} \qquad\qquad \text{etc.} \end{array}$$

We have only to assume that k, l, etc., are sufficiently near zero (so that K, L and all other rays affected with such coefficients sensibly vanish before they reach the observer), to see that the only quantities sensible to observation are those with relatively large coefficients as A, B, C, D, etc., so that now

$$\begin{aligned} Aa + Bb + Cc + Dd + \text{etc.} &= M \\ Aa^2 + Bb^2 + Cc^2 + Dd^2 + \text{etc.} &= N \\ Aa^3 + Bb^3 + Cc^3 + Dd^3 + \text{etc.} &= O \end{aligned}$$

From these values M, N, O, etc., it is plain that we can never estimate the amount of the extinguished rays K, L, etc., since these do not enter into the observed values by any amount sensible at all.

Now to the rays A, B, C, D, etc., which remain, and to their coefficients, we may evidently assign any values consistent with the conditions, which shall make the difference between $\frac{M}{N}$ and $\frac{N}{O}$ as small as we please, for in the equations

$$\frac{Aa + Bb + Cc + Dd + \text{etc.}}{Aa^2 + Bb^2 + Cc^2 + Dd^2 + \text{etc.}} - \frac{Aa^2 + Bb^2 + Cc^2 + Dd^2 + \text{etc.}}{Aa^3 + Bb^3 + Cc^3 + Dd^3 + \text{etc.}} = R$$

we can always assign positive and real values to A, a, etc., such that R may be as small as we desire.

But R represents here the apparent error of observation as inferred from a comparison between "high and low stars." Hence it follows that *however close the agreement may be between observations on absorption, made at quite different altitudes of the heavenly body, we have no right to infer that the error of the final result is not indefinitely great.*

If while the truth of the above proposition be admitted in the abstract, it is still urged that we do not, as a matter of fact, have reason to suppose that so indefinitely large a part of the solar rays are quite extinguished ere they reach us, I would point out that it is not in reality necessary to suppose the present extreme case (*i. e.* that of a large part of the original radiation being wholly absorbed), which has been taken here only to make the nature of the argument more evident.

For if we agree (as we certainly may) that a notable portion of the coefficients are near zero, and another notable portion still but small fractions of unity, what we have just shown for an extreme case will also hold for the usual one, for it follows from the previous demonstration that the greater the discrepancy between the coefficients, the more shall we underrate the true absorption and the greater will be our error. To see what the conditions actually are, in inferring it from the ordinary formula, we must now consider more narrowly how this tellu-

ric absorption takes place. I have already spoken of the general, or non-selective absorption, whose extreme type is the scattering of light by large dust-particles in a sun-beam, and now proceed to consider the other typical extreme, which is that of purely selective absorption.

I have here some photographs* which I owe to the kindness of Professor Rowland of Baltimore, in which we have a portion of the spectrum near D, photographed when the sun was on the meridian, and a second photograph of the same limited portion at about half-past three in the afternoon, when the air-mass traversed was only about one-half greater.

Notice, nevertheless, the immense difference caused by the growth of telluric lines in this short interval. There is scarcely a hair's breadth of the plate, which they have not invaded. It is true the whole spectrum is not so densely crowded with them, as this region is, and yet, broadly speaking, we may say that almost the entire spectrum is visibly filled with telluric lines, in all but juxtaposition, just before sunset.

What is a telluric line? A very narrow, black and cold region, where the absorption has already done its full work, or which is at any rate so black and so cold, that it can grow very little blacker or colder. The extinction of the ray here is nearly absolute, or in other words, its coefficient of transmission is very small indeed. If we consider the same part of the spectrum at noon, we find that the region occupied by these lines must in reality be darker than if there were no absorbing air, even if the absorption has not progressed so far that the individual absorption lines are visible, or distinctly black. But in fact we do see parts of them distinctly black even at noon. Moreover, if we climb a mountain into the upper air, we find great numbers of these rays practically extinguished even there, long before they have reached the observer at sea level. In this way, I have myself observed numerous telluric lines quite black on Etna,† and even at greater altitudes, in the pure dry air of the Sierra Nevadas of California, so that there is much reason to believe that at the highest altitudes attainable by man, certain portions have already disappeared from the spectrum, and that we cannot correctly infer the original condition by any amount of observation on the different rates of absorption of what remains. Irradiation makes the telluric line appear narrower and weaker than it really is. Photography in this respect is probably a more trustworthy guide; and I think that those of us who are used to seeing the spectrum of a low sun, and who have gathered any impression of thinness and rarity of these lines, may correct our ideas with advantage, by the study of these admirable

* Not given here.

† See also the important observations by Professor Smythe on Teneriffe.

photographs, of which I will only observe, that when they were taken, the air-mass at noon was 1·09 and in the afternoon, 1·60, so that all this increase of telluric lines, came with a very little increment of the absorbing air, and is but a small part of what we should see nearer sunset. Evidently the noon spectrum must be less bright, not only for the telluric lines distinctly seen, but for those indistinctly seen individually, or latent only, and which come out as separate individual lines when the sun is lower. It results from what has just been said, then, that the part of the absorption due to telluric lines alone, is more considerable than is commonly thought, and that the coefficients of absorption and transmission, which have been obtained by various writers on actinometry and photometry, are really in general the mean coefficients obtained from the average of hundreds of these alternations (and necessarily too small), and yet more than this, that the smallest part included in the field of the experiment, whether the telluric lines are separately visible, or whether they are only latent there, is filled with alternations of transmission and absorption, and therefore, according to our previous demonstration, the mean result, even when obtained by a linear thermopile or bolometer, must still indicate too feeble an absorption. I speak now only of the strictly selective absorption, but I again remind the reader that there is also the partly selective and comparatively non selective absorption already mentioned, and that, practically, between the telluric line and the general absorption, we have every intermediate coefficient of transmission from unity to zero. The previous criticism applies then, though in a less degree, to the few investigations where two or three coefficients have been used, and even to investigations with the linear thermopile or bolometer, using numerous coefficients, among which I mean to include my own. I have done all I could to minimise the error by measuring on rays as nearly homogeneous as possible, that is, by measuring on parts of the spectrum so narrow, that they may be called, without exaggeration, linear, a name which we may certainly apply to a hair-like line $\frac{1}{5}$ of a millimeter in width, forming the working face of the linear bolometer; but even this strip, when laid down in a considerably dispersed spectrum, covers more than the distance between the D lines; and if we fix our attention on that well known region as a type, we see that this hair-like line itself covers in this narrow interval alone, at least a dozen alternations between brightness and almost total extinction, so that though in respect to wave-lengths we may be said to measure approximately homogeneous rays with a linear instrument, in respect to this local absorption, we do not. I am convinced that we do not know what this absorption really is in amount, but I

think we can now begin to see somewhat of what it is in kind, and may be prepared to agree that the data in the annexed table (table II) may represent numerically the proportions of nature, with a certain approximation. In this table (II) we have certain numerical results consequent on the approximative hypothesis that the total heat in sun or star is divided into a certain finite number of parts, each one of which has its own rate of absorption.

Here the radiant energy before absorption is supposed to be divided into ten parts, A, B, C, ... J, each having its separate coefficient of transmission a, b, etc., an arrangement which may be taken to represent some sort of adumbration of the complexity of nature's problem, and the method of her work. It is given here only in illustration of the kind and degree of the errors induced by use of the usual formulas, and the demonstration already given in no way depends on these numerical values, whose exactness I do not need to maintain, since they are offered merely in justification of the previous assertion, that the absorption at any rate *may* be more than double what we think it (as in this specific case) without our having means of knowing of our error through our present formula.

It may, however, be incidentally observed that these values do typify the facts, with a certain approximation to the real values of nature, for they are obtained by approximate solution of equations of the form

$$\begin{aligned} Aa + Bb + Cc + Dd + Ee \text{ etc.} &= M \\ Aa^2 + Bb^2 + Cc^2 + Dd^2 + Ee^2 \text{ etc.} &= N \\ Aa^3 + Bb^3 + Cc^3 + Dd^3 + Ee^3 \text{ etc.} &= O \end{aligned}$$

where M, N, O, etc., do not differ widely from the results of actual observation. The conclusion in this specific case therefore seems fairly typical of that in the general one.

We have here supposed that the radiant energy from the sun or star before it enters our atmosphere is divided into ten equal parts, each of which in general suffers some different partial absorption. While no ray may be absolutely absorbed or wholly transmitted, a certain small part (represented in the spectrum by known telluric lines) is so nearly absorbed, that its coefficient in the first decimal place would be 0, and a certain more considerable portion, corresponding in a general sense to certain infra-red rays, has coefficients here undistinguishable from unity. Probably the greater part of the spectral energy however is intermediate between these two extreme types, and so our numerical values indicate.

The first column is the original intensity before absorption. (We have in this particular example, for simplicity, supposed

$A = B = C \ldots\ldots\ldots = J = 1$, though this condition is not necessary. It will be observed, however, that under it in the second column $Aa = a$, $Bb = b$, etc., so that the coefficients of transmission, the ratios of each geometric progression, are the same in this particular column, as the intensity after absorption.)

TABLE II.

I. Original intensity (ordinarily unknown.) A, B, C, etc.	II. Observed intensity after one absorption. Aa, Bb, Cc, etc.	III. Observed intensity after two absorptions. Aa^2, Bb^2, Cc^2, etc.	IV. Observed intensity after three absorptions. Aa^3, Bb^3, Cc^3, etc.	V. Observed intensity after four absorptions. Aa^4, Bb^4, Cc^4, etc.
1	·01	·0001	·000	·0000
1	·1	·01	·001	·0001
1	·2	·04	·008	·0016
1	·6	·36	·216	·1296
1	·7	·49	·343	·2301
1	·7	·49	·343	·2301
1	·8	·64	·512	·4096
1	·9	·81	·729	·6561
1	·9	·81	·729	·6561
1	1·0	1·00	1·000	1·0000
10= A + B + etc. = X	5·9= $Aa + Bb$ + etc. = M	4·65= $Aa^2 + Bb^2$ + etc. = N	3·881= $Aa^3 + Bb^3$ + etc. = O	3·3143= $Aa^4 + B{\cdot}b^4$ + etc. = P

If we determine the coefficients of transmission from a comparison of II and III, we have $\frac{N}{M} = ·789$;

if from II and IV, we have $\left(\frac{O}{N}\right)^{\frac{1}{2}} = ·812$;

if from II and V, we have $\left(\frac{P}{O}\right)^{\frac{1}{3}} = ·825$

and the corresponding mean absorptions are

$$1 - \frac{N}{M} = 0·21, \quad 1 - \left(\frac{O}{N}\right)^{\frac{1}{2}} = 0·188, \quad 1 - \left(\frac{P}{O}\right)^{\frac{1}{3}} = 0·175.$$

So that all our observations at very different altitudes are in substantial agreement in indicating an absorption of from 18 to 21 per cent, while yet all our inferences from them are quite wrong.

If we observed by some method which discriminated between the different radiations of which the heat or light is composed, we should have from the observations in columns II and III

$$A + B + C + \ldots\ldots J = \frac{(Aa)^2}{Aa^2} + \frac{(Bb)^2}{Bb^2} + \frac{(Cc)^2}{Cc^2} + \frac{\ldots\ldots (Jj)^2}{Jj^2} =$$

$$\frac{·01}{·0001} + \frac{·1}{·01} + \frac{·2}{·04} + \text{etc.} \ldots\ldots = 10$$

(the true value) while the ordinary and erroneous method, which does not discriminate, gives

$$A+B+C+\ldots=\frac{(Aa+Bb+Cc+\ldots)^2}{Aa^2+Bb^2+Cc^2+\ldots}=\frac{(5.9)^2}{4.65}=7.5 \text{ nearly.}$$

We give the most favorable case for the observer, where (what is rarely or never actually possible), he begins his observation with the sun or star in the zenith, in a sky so changelessly serene that he may continue them up to a point where secant $\zeta=4$ (corresponding to column V), and where the altitude is less than 15°; and we here suppose ourselves to have a prior knowledge both of the actual amount of the light or heat before absorption, and of its composition, while he is assumed to attempt to deduce the prior amount by the usual formula, and from the usual observations, made in the gross by the photometer or actinometer.

The successive values of the absorption thus found by comparing a zenith observation with three successively lower altitudes, are 21 per cent, 19 per cent, 18 per cent. All agree much within the probable error of actual observation, as observers conversant with this matter will readily admit, and yet the true value is all the while

$$1-\frac{5.9}{10.0}, \text{ or 41 per cent.}$$

It will have been noticed in fact, that the determinations of this absorption-coefficient by various observers already cited, differ among themselves as much as these values do from each other, and if these conditions represent those of nature, the result must be in practice, that years of observation will be accordant in giving the wholly wrong absorption of from 19 to 22½ per cent, and that the actual minute systematic discrepancies pointed out by our theory, and which are significant of some error in the formula, would probably remain long undetected. While the observer, then, we admit, has strong apparent evidence from the close agreement of his observations, that if there be an error in his formula, it is practically negligible, yet this evidence according to our demonstration is fallacious, and the actual error, as appears from the numerical illustration, *may well exceed double the amount* in question, for the above values might be increased without imposing any conditions but such as it may be reasonably assumed are those of nature.

The writer believes the actual mean absorption of sun and star-light to be not improbably over 40 per cent, at the sea level; but were the stars alone in question, the fact would have but little importance, since their relative magnitudes (unless

considerable color is present) remain nearly the same with the false hypothesis as with the true one, and it is with their relative magnitudes that the student of stellar photometry is chiefly concerned, for he desires to know their relative brightness at the zenith rather than their absolute brightness outside the atmosphere.

With the sun, however, it is otherwise, for here it is the absolute heat or light which is in question.

Accordingly, when we apply our above conclusions either to problems of solar physics or of meteorology, the result is of an altogether different importance. Almost all the phenomena of meteorology would become predictable if we knew how much heat reaches the soil, and how much, and in what altered kind, is returned to outer space. To solve these problems we must know how much is absorbed by our atmosphere; and there are further reasons, independent of those cited, for believing that this may be more than double what is commonly supposed.

It may be observed that the comparison of observations at the base and summit of a very high mountain will enable us to obtain much better determinations than the method of high and low sun observations, not only on account of the diminution of the absorbing air-mass, but because we thus avoid certain systematic differences between the atmospheric conditions at noon and evening (or morning) which introduce constant errors into the results, in addition to those already considered. From such observations I have been led independently of theory to conclude that the absorption is greater than is commonly supposed. But beside this method, which is in reach of but few, there is another at the command of all, the significance of whose results seems to have been hitherto overlooked.

If we are willing to agree that most of the solar heat is not absorbed by our air in the sense of being accumulated there (since this would heat the atmosphere to the condition of a glowing gas) we must, it seems to me, admit that it is mainly diffused toward and away from us by particles, so that nearly all we lose by selective absorption from the direct light and heat of the sun, we ought to find reflected or selectively diffused to us from the sky, or turned away from us to outer space.* I have been engaged in measuring the heat and light from the sky around the sun, and that from the whole sky apart from the sun. These measures are still incomplete, and I will only say that they seem to show not only that the average amount of blue light (to speak for the moment of blue light only) which

* See the investigation of Tyndall on the cause of the blue color of the sky, Proc. Roy. Soc., vol. xvii, p. 223. See also the theoretical investigations of Clausius, Poggendorf Annalen, vol. cxxix, p. 330, *et seq.*, and of Lord Rayleigh, London, Edinburgh and Dublin Phil. Mag., Feb., 1871, *et seq.*

is thus scattered from an ordinary pure and cloudless sky at the sea level, already represents a selective absorption of very much over 40 per cent of the original blue in the direct sunlight; but that also the *mean* diffusion is, though less than this, still probably over 40 per cent, and hence that to obtain the actual light of sun or star before absorption, we must probably add over 40 per cent to the observed value. To make the propriety of this last statement clear, it may be observed that if there were bright clouds in the sky, we should have (as we know by experience) more light from the clouds, than from an equal portion of the blue, but that in this case, our observing station would gain the added light at the expense of those portions of the earth which were in the clouds' shadow, and in this case, therefore, we should not be justified in adding the light we receive to the observed sunlight to obtain that before absorption. But with a uniform sky, it follows that every point on the horizon enjoys the same sunshine that we do, at our own station; and here it is evident that if the atmosphere were taken entirely away, the sun would grow brighter by all the light which the atmosphere now sends us, and, in fact, by more, since this atmosphere is scattering light not only toward us, but also to outer space.

Roughly speaking we may say, that we receive on the average at the sea level, as much light from the sky as we do from the sun itself; getting more light from the sun at midday than from the sky, but more in the morning and afternoon from the sky than from the sun. There is no reason to believe that any smaller proportion of star-light is diffused earthward, and this is independent of that which is diffused toward outer space.

All my own investigations, whether through observations at the sea level or at an altitude of nearly 15,000 feet, lead me to believe it probable that the mean absorption of light (and of heat also) by our atmosphere is *at least double* that which is customarily estimated, and also to conclude that fine dust particles, both near the surface and at a great altitude, play a more important part in this absorption, both general and selective, than has been heretofore supposed. I hope soon to make public the result of an investigation into the composite character of the radiation, as an indication of what the sky has selectively borrowed from the direct solar rays.

Art. XXIII.—*Tornadoes;* by Henry A. Hazen.

"The true tornado," says R. H. Scott, "occurs off the west coast of Africa and is identical with the arched squall of other waters." This restrictive definition of a tornado is not accepted generally in the United States, where it is applied to an intense, seemingly local, outburst, ordinarily preceded by a funnel-shaped cloud, having a rapid rotation and a more or less slow up-and-down movement. The better designation would undoubtedly be "whirlwind," but the term tornado has become so well understood that it hardly seems wise to attempt a change.

The importance of a proper study and a good knowledge of the forces, which underlie the formation of a tornado, will be easily recognized when we consider the enormous loss of life and property that is annually occurring in the west and south. As the western States become more thickly settled year by year, it is manifest that this loss of life will become greater unless means for individual safety are provided in cellars and dug-outs as already recommended, or some more general means be employed for destroying or diminishing the force of the storm as it approaches a large city. Such an idea may seem chimerical; but if Franklin in the eighteenth century devised so simple and effective a means as the lightning rod for protecting buildings from lightning, surely the genius of the nineteenth century may determine some means of mitigating, if not of wholly avoiding, the evils under consideration. The devastation of large villages and the great terror induced at the possible approach of a tornado in a heavy dark cloud are sufficient reasons for the closest study of these phenomena.

It is the object of this paper, first, to set forth some of the ordinary theories that are advanced for explaining the origin and development of these outbursts; secondly, to show some of the seeming difficulties in these theories; and thirdly, to point out a few of the characteristics of these outbursts and to attempt to show lines of investigation upon which a further advance may be made toward a true knowledge of the forces underlying them.

Unfortunately, their origin is involved in much obscurity, due in part to a lack of observation of the conditions immediately preceding the sudden and destructive manifestations. The following quotations from authorities will suffice to show some of the views at present entertained. One writer says: "The inward rush of winds toward a depressed center is the cause of our thunder-storms, which are only infant cyclones and tornadoes. The whole country for 500 miles square from the Missouri to the Ohio valley is covered with a mass of warm moist

air flowing northward. At numerous spots in this region this acquires an upward motion, thereby giving rise to local upward currents of air which cool rapidly as they rise. The cooling is a mechanical result of the expansion of the rising air and very soon a temperature is reached low enough to condense clouds from the hitherto invisible moisture. With the formation of clouds, the tendency to rise increases, so that in fact an upward suction is experienced under the cloud and more air is drawn in from all sides to feed this suction."

"Whatever causes a sudden uprush of moist air contributes to the formation of the cloud or the tornado. Hills or low mountains are very effective. But it is equally important to consider the cool dry air that flows from the north toward a low center and becomes a west wind as it turns around the low, runs into the mass of warm moist air coming from the south, and being denser, underruns and lifts up this warm air and is in many cases more effective than a mountain in starting the formation of a cloud and local storm."

Another writer gives a theory directly the opposite of this last and says: "Suppose a mass of warm, moist southerly wind has pushed itself below a colder northwesterly stratum. The warm wind, feeling about for a point of escape through its cold cover, soon makes or finds a vent where it can drain away upward, and then the entire warm mass even a mile or more in diameter and often more than one thousand feet in thickness, begins a rotary motion, rises at the center and passes away."

It is probable that theories upon the effect of the sun's heat upon the lower atmosphere have been at fault. It has been assumed that "this heat is developed in certain central portions of greatest heat." It is ordinarily considered also that the heat at the earth's surface is greatly increased and causes masses of heated air to rise to great heights. We cannot determine precisely the area uniformly heated by the sun during the hottest part of the day; however the temperature is well nigh at a maximum for at least an hour. This would give a circle of heated air over one thousand miles in diameter, which, if inland and at sea-level, would be heated with nearly equal intensity on clear calm days. Experiment shows that, while the earth's surface in sunshine becomes very hot, yet this heat does not extend to more than two or three inches in the atmosphere. At six inches the temperature is frequently from thirty to forty degrees less than at the surface, and from this point it diminishes not more than one half a degree in six feet, showing that there is no appreciable rising of heated air from the earth's surface.

The effect of the sun's heat may be considered as follows: the stratum of air nearest the earth will first be heated and

gentle convection currents set in motion, these will heat the next stratum, and so on.

This is the explanation given by Prof. Hann of Vienna, who futhermore considers the formation of cumulus cloud in summer to be due to the action of vapor immediately at a height where the dewpoint is reached, but that no great interchange of air from the lower to the upper atmosphere will take place from the action of heat alone.

Again, the evaporation and condensation of moisture are advanced as important factors; it is stated for example: "Of great importance is the action of vapor, as a great storehouse of solar energy, required in the process of its evaporation, generally known as 'latent heat,'" and "condensation is attended with the production of just as much heat energy as was lost in the process of evaporation." Admitting for a moment that a warm vapor-laden current of air has found its way to some height above the earth, where is the evidence that a condensation will produce any more forcible effect than would have been noted in evaporation? If the process of evaporation goes on by insensible degrees, why may not the converse process of condensation take place in the same way? It would seem a hasty conclusion that a mass of vapor can be cooled and condensed so much more rapidly than it can be evaporated. Either process requires time and certainly that of cooling a large mass of air can only be accomplished slowly. That a rapid upward rush and consequent expansion would cool the air is evident, but it is by no means certain that there is this uprush, and again that the cooling would produce any sudden display of force. The fact that efforts at a computation of the probable liberation of energy arising from such action have given most diverse results, with even an absolute denial of any sudden release of energy, shows that such an hypothesis must be accepted with caution and should be supported by indubitable practical tests.

The theory that tornadoes occur where there are great contrasts of temperature in warm southerly and cooler northerly winds is not supported by the facts. This theory is partly based on the assumption that tornado action occurs near the center of a low area. It is admitted that immediately to the north and south of a storm center there are strong contrasts of temperature and there is a partial meeting of warm and cool winds, but there are no tornadoes at or even near that point.

Let us take a map of any region having upon it, very near the time of the tornado, lines of equal pressure and temperature and directions of the wind. There will invariably be upon it a storm-center. Through the center of this draw a line from southwest to northeast and we shall find on the north-

west side low temperature and cold and dry northwest winds, due to an advancing high area. On the southeast side, however, the air is laden with moisture and is hot, there are frequent rains, and well nigh uniform south and southeast winds. On the southeast side also the isotherms are much farther apart than on the northwest side. If now we project the tornado tracks upon the map we shall find them in the region to the southeast, and generally hundreds of miles from the storm-center, where there are no northerly winds. A critical study of tornado tracks upon a weather map will develop these points in nearly every instance. Frequently there are in the tornado region higher temperatures in the north than in the south, and very seldom will there be a lowering of temperature from south to north any greater than the constant difference due to the difference in latitude.

Again, there seems to be a disposition to advance some general and sweeping theories about cold or hot winds underrunning hot or cold winds, etc. It may be said that, in a condition of equilibrium, the cooler air must flow beneath and, at all times there would be a more or less insensible diffusion of the two masses. It would seem difficult to account in this way for the definite formation of a tornado and its onward advance dipping here and there sometimes even across a state.

In seeking for a probable solution of the problem, and in testing the assumptions made, we have to-day a powerful adjunct in the weather map showing at any hour of the day the precise meteorological conditions existing over the region of tornado action. Such maps for three nearly equidistant hours of the day have been published by the U. S. Weather Bureau from 1872 to 1880. In order to ascertain the prominent facts connecting tornado action with storm centers there have been selected for this paper forty-one of the tornadoes described in Sergeant (now Lieut.) Finley's paper, "Characteristics of 600 tornadoes," also published by the Weather Bureau. Before the tracks were projected upon the maps, every tornado having special destructive action and at the same time a fairly well determined note of time, was tabulated and of these none were left out in the final result. Tornadoes between September, 1872, and September, 1879, have alone been considered.

TABLE.

Place.	Lat.	Long.	Date. Hour.	Distance (miles) to and direction from low center			Temperature falls 10° in miles.	Winds near tornado.	Distance to nearest N. wind.	Pressure at low center.
Charlotte, N. C.	35·2°	80·8°	Sept. 8, '72, 5 p. m.	650	S.E.	135°	500	S. & S.E.	650	29″·76
Keokuk & Washington Co's, Ia.	41·3*	92·0*	May 22, '73, 2.15 p. m.	440	S.E.	135	200	S. & S.E.	370	29·28
Cloud Co., Kans.	39·6	97·7	June 29, '73, 3 p. m.	480	S.S.E.	158	520	S. to E.	520	29·60
Cairo, Ills.	37·0	89·2	Mar. 18, '74, 4 a. m.	370	E.S.E.	112	210	S. & S.E.	260	29·76
DeSoto Co., La.	32·0	93·6	Apr. 7, '74, 4 p. m.	260	E.S.E.	112	140	S.E.	220	29·70
Butler Co., Kans.	37·8	96·9	June 22, '74, p. m.	660	S.S.E.	158	400	S. & S.E.	none†	29·44
Troy, N. Y.	42·7	73·6	Aug. 2, '74, p. m.	580	S.S.W.	202	260	N. & N.W.	----	29·55
Dixon, Ills.	41·8	89·5	Aug. 10, '74, p. m.	620	S.E.	135	200	S.W. to S.E.	none†	29·33
Montevallo, Ala.	33·1	86·9	Nov. 22, '74, p. m.	490	S.E.	135	130	S.	670	29·04
Fulton, Miss.	34·2	88·3	May 1, '75, p. m.	600	S.S.W.	202	120	N.W.	----	29·10
St. Joseph Co., Ind.	41·6	86·3	June 1, '75, p. m.	610	S.E.	135	150	S. to E.	none†	29·31
Quincy, Ills.	39·9	91·4	June 14, '75, p. m.	540	E.S.E.	112	110	S.W. to S.E.	350	29·41
Omaha, Nebr.	41·3	95·9	June 17, '75, p. m.	270	S.E.	135	160	N.E.	----	29·60
West Point, N. Y.	41·3	74·0	July 13, '75, 7.10 p. m.	420	S.	180	150	S.W.	390	29·54
Salem Co., N. J.	39·5	75·3	July 29, '75, 3 p. m.	550	S.E.	135	100	S.W. & S.	290	29·72
Lomonauk, Ills.	41·6	88·7	Aug. 5, '75, 7.20 p. m.	210	S.E.	135	390	S. & S.W.	410	29·50
Milford, Pa.	41·3	74·8	Oct. 30, '75, p. m.	350	S.E.	135	270	S. & S.W.	800	29·53
Chicago, Ills.	41·9	87·6	Apr. 26, '76, 5.10 p. m.	780	S.E.	135	210	S. & S.W.	none†	29·41
Carbondale, St. Clair Co., Ills.	37·8	89·2	May 6, '76, 9.30 p. m.	360	S.S.E.	158	240	S. & S.E.	200	29·41
Saline Co., Kans.	38·8	97·7	June 6, '76, 3.30 p. m.	480	S.S.E.	158	400	S.W. to S.E.	600	29·45

* Lat. and long. of center. † No north wind.

TABLE—CONTINUED.

Place.	Lat.	Long.	Date. Hour.	Distance (miles) to and direction from low center			Temperature falls 10° in miles.	Winds near tornado.	Distance to nearest N. wind.	Pressure at low center.
Washington Co., N. Y.	43.2°	73·2°	May 18, '77, 1.30 p. m.	220	S.S.W.	202°	200	S. & S.W.	400	29″·59
Memphis, Tenn.	35·1	90·0	June 17, '77, 7 p. m.	1040	S.E.	135	1000	S. & S.E.	650	29·28
Elkhart, Ind.	41·7	86·0	July 2, '77, 4.30 p. m.	1030	S.E.	135	200	S. & S.E.	580	29·58
Council Bluffs, Ia.	41·3	95·6	Aug. 6, '77, 2.30 p. m	520	S.S.E.	158	580	S. & S.E.	190	29·75
Fayetteville, N. C.	35·0	78·9	Feb. 8, '78, 7 a. m.	420	S.E.	135	420	S. & S.W.	430	29·30
Cairo, Ills.	57·0	89·2	Ap. 23, '78, 4.10 a. m.	400	S.S.E.	158	240	S.E.	350	29·43
Carolina Landing, Miss.	33·0	91·1	May 18, '78, p. m.	720	S.E.	135	120	S.	490	29·28
Barrington, Ills.	42·2	88·1	May 23, '78, 6 p. m.	600	S.E.	135	130	S.W. to S.E.	340	29·32
Cash Co., Mo.	38·6	94·3	July 16, '78, 2 p. m.	420	S.	180	700	S.	350	29·54
South Hartford, N. Y.	43·3	73·2	July 26, '78, 5.30 p. m.	290	N.E.	45	90	S. & S.W.	270	29·29
Wallingford, Conn.	41·5	72·9	Aug. 9, '78, 6 p. m.	480	S.	180	150	S. & S.W.	300	29·40
Near Dover Mines, Va.	37·7	77·9	Sept. 12, '78, 4 p. m.	300	N.E.	45	160	E. & S.E.	300	29·48
Monticello, Jones Co., Ia.	42·2	91·2	Oct. 8, '78, 5.30 p. m.	320	E.N.E.	68	90	S. & S.W.	260	29·54
Collinsville, Ills.	38·7	90·0	Apr. 14, '79, 2.45 p. m.	160	S.S.W.	202	100	N. & N.E.	----	29·50
Dallas, Tex.	32·8	96·8	Apr. 15, '79, 2 p. m.	240	S.E.	135	100	E. & N.E.	----	29·34
Walterboro, S. C.	32·9	80·7	Apr. 16, '79, 3.45 p. m.	320	S.E.	135	210	S. & S.E.	420	29·47
Barnard, Mo.	40·2	94·8	May 29, '79, 3.30 p. m.	420	S.	180	300	S.W. & S.	600	29·29
Lees Summit, Mo.	38·9	94·3	May 30, '79, 5.30 p. m.	260	E.	90	240	S.E. & S.	330	29·40
Delphos, Kans.	39·3	97·8	May 30, '79, 4 p. m.	60	E.N.E.	68	150	N. & N.W.	----	29·40
Erie Co., O.	41·4	82·7	July 11, '79, 3 p. m.	230	S.W.	225	300	S. & S.W.	300	29·46
Sandy Spring, Ind.	39·2	77·0	Sept. 3, '79, 6 p. m.	420	S.E.	135	270	S. & S.W.	510	29·33
Means	----	----	----------	453	S. 39° E.	141	259	---- ----	----	29·45

In this table, column six gives the distance in statute miles to the storm-center; the next two columns contain the direction of the tornado from "low," the numbers indicate the number of degrees from the north counting by east. Column nine shows the approximate distance between isotherms ten degrees apart, or the distance giving a temperature fall of ten degrees. This column is presented here as only a rough approximation; it was found necessary frequently to take the line of measurement at right angles to the line toward "low," and the measurements are vitiated by great irregularities in the conformation of the isotherms. Column ten contains the general course of the winds in a large region surrounding the tornado. Column eleven gives, as nearly as possible, the distance to the nearest north wind; this was seldom measured in the direction of "low;" if it had been, the distances would have been much greater. The last column contains the lowest pressure at the center. This table brings out many most interesting facts and merits careful study.

It has been assumed that a definite relation exists between a "low," and the place of tornado action. Certain it is that no extensive tornado action ever takes place without the presence of a "low" of rather marked intensity. The probable influence of "high areas" is for the present neglected.

A general average of this table gives the mean distance from "low" 453 miles; mean direction, S. 39° E.; mean temperature fall, ten degrees in 259 miles. The winds are almost uniformly from the south and southeast, and if from any other quarter, all are from that direction. The distances to the nearest north wind are variable, in many instances there were no north winds on the map near the "low" or the tornado; the mean distance in thirty-one cases was 407 miles.

We may conclude then, first, that in the region of tornado action there is no upward rush such as is supposed to take place at a low center; secondly, that the winds are uniform and there is no meeting of cool northerly with warm southerly breezes; thirdly, that there are no contrasts of temperature, the fall of ten degrees in 259 miles being no more than would be expected from the influence of latitude.

Another most important point is the existence of extensive hail and thunder storms in the neighborhood of the tornado, which would seem to show an intimate relation between the two.

It has been commonly accepted that thunder-storms are local phenomena, and that they are caused by heated air rising on warm and sultry days and condensing its moisture in the upper regions. The facts, however, seem to show a much more particular cause. Take a very recent instance, the earliest exten-

ded thunder-storm of this year in the eastern United States. This occurred February 14, 1884, and was first noted at Augusta, Ga., early in the morning. It appeared at Lynchburg, Va., at 4 A. M., and at Variety Mills, Va., about the same hour, where it seemed to come from southwest and passed off to east-northeast. It passed over Washington, D. C. from west-southwest at 6.25 A. M. toward east-northeast. It was noted at Haverford College, Pa., and at New Castle, Del., at 8.30 A. M. These times (about some of which there is a little uncertainty) give the following velocity: Lynchburg to Washington, about sixty-two miles per hour; Washington to Haverford College, the same. The velocity across Washington, as determined by counting the seconds between the flash of lightning and the thunder, on the west as it came up and on the east as it passed off, was a little less than a mile a minute.

It may not be that one and the same storm passed across this whole region, though the evidence favors that supposition. One thing is certain, namely, that the meteorological conditions were suited to just such a development of thunder-storms over a large extent of country. It seems probable that they could not have been caused by the rising of moist heated air in a single locality. It is believed that no thunder storm can occur without a storm-center or disturbed region existing somewhat near it. This center may not make itself known by distinct bendings of isobars, but it can be easily recognized by the presence of cloudy weather and rainfall, with more or less incurving of the winds. An attempt has been made to divide thunder-storms into two classes—first, those caused in the presence of a low center of pressure, and, second, those due to the sun's heat. It may be admitted that the sun's direct heat is an important factor in the formation of summer thunder-storms, but this only acts in conjunction with the disturbed region which is invariably present.

If, now, such a thunder-storm action is general, and is dependent upon the presence of a storm-center, may we not hope by careful observation at short intervals of time and space, in the region over which the tornado action is expected to occur, to obtain a sounder basis of fact to reason upon than we now have?

One step further we may venture. If we can correlate thunder-storms and storm-centers, may we not expect to obtain by a critical study of the former some evidence of the forces acting in producing the latter? By such critical study we ought to be able to obtain answers to the following, among the questions relating to tornadoes:

At what distances and in what directions do they lie from the low center? How are they distributed with respect to thun-

der-storms? How are they connected with each other and what is their movement? What wind directions prevail? What is the distribution of temperature? Is their relation to storm-centers an invariable one? What is the character and distribution of hail storms? and so on. As a partial answer to the last question, observation seems to establish a rule that hail storms occur only in immediate proximity to storm-centers of considerable energy.

In order to be able to predict violent local storms and tornadoes we can accomplish much, as has just been suggested, by a study of a map containing the weather conditions over a large region.

Some of the signs of probable tornado action may be enumerated, in conclusion, as follows:

First—There must be a storm-center fairly well marked, though this may be 600 or 700 miles away. Tornadoes almost invariably occur to the south and southeast of this point.

Second—It is believed that the temperatures in the above region should be above the mean. Great contrasts of temperature are not necessary, and if, in connection with these, an area of high pressure is found to the northwest or west, no tornado will ordinarily occur.

Third—In general the air should be moist.

Fourth—As extended thunder-storm action is an invariable accompaniment of a tornado, it is believed that these may give some indications of more severe action. It is not thought that the above will prove an infallible guide, but ordinarily no tornado will occur unless the above conditions precede it.

Prof. Tait of Edinburgh has recently published an article on the origin of atmospheric electricity, in which he takes grounds similar to those of certain French savants, namely, that as yet we know little or nothing of the origin of this manifestation of electricity. It would seem that J. Allan Broun's theory that it is due to the direct influence of the sun's electricity upon the moisture of the air, or possibly to the indirect effect from the sun's heat is more satisfactory than the multifarious theories of friction, evaporation, condensation, etc. However this may be, it seems that the present theories on the origin of tornadoes are no more satisfactory than those on the origin of atmospheric electricity which are abandoned by Professor Tait. Certainly as all theories in regard to lightning have proved inadequate upon actual tests, so it should be insisted upon that no mere paper theory of tornado action can be accepted.

ART. XXIV.—*On the Absorption of Radiant Heat by Carbon Dioxide;* by J. E. KEELER.

AMONG the gaseous bodies whose power of absorbing radiant heat has engaged the attention of physicists, one of the greatest interest is carbon dioxide (CO_2, commonly called carbonic acid gas), since as a constituent of our atmosphere, its action has an immediate bearing upon matters whose importance to us is evident. It would seem at first sight as if the determination of the absorptive power of a gas were a matter of no great difficulty; yet this is so far from being the case that most conflicting results, especially in regard to the absorption of water vapor, have been reached by different investigators, the discussion between Tyndall and Magnus on this subject being well known. Quite recently the question has been revived, "In which of the gases composing the atmosphere does the power of absorbing the sun's rays lie?" And, indeed, when we consider that, according to measurements made by different observers at different times and places, the action of *any one* of the principal constituents of the atmosphere is sufficient to account for this absorption, it can hardly as yet be regarded as settled. That carbon dioxide has a remarkable power of absorbing radiant heat has long been known, and within the last few years, methods of analysis based upon this property have been invented.

The usual course pursued in investigating the subject is to allow radiant heat to pass through a stratum of gas of given thickness, and measure the amount stopped by it by comparing the indication of a radiometric apparatus placed opposite the source of heat before and after the interposition of the gas. In this way (considering the more recent experiments), E. Lecher and J. Pernter,* using as a radiant source a metal plate heated to 100° C. by steam and a gas stratum 31^{cm} thick, found that whereas neither dry nor moist air produced any absorption whatever, the action of carbon dioxide was very marked; from which they concluded that the absorption of the air cannot be ascribed to aqueous vapor which it contains, but must be attributed to carbon dioxide, which, according to their measurements, exists there in sufficient quantity to produce the absorption of the sun's rays indicated by the actinometric observations of Violle. Upon this conclusion Dr. Lecher† based a method for determining the percentage of CO_2 in the atmosphere, which he afterwards tested by observations upon the Alps. The method consists, in brief, in measuring the intensity of the sun's rays at different hours of the day, before and after passing through a tube of variable length filled with CO_2, the final result being

* Wied. Annalen, xii, p. 180. † Wied. Annalen, xii, p. 466.

given as the number of meters of pure CO_2 at the ordinary atmospheric pressure through which the sun's rays if vertical would pass in reaching the earth, the ratio of this to the depth of the homogeneous atmosphere giving at once the percentage of CO_2 required. It was to test the validity of this method, (which however is of little practical value, since it is only applicable in regions where the purity and constancy of the sky is quite exceptional,) and in the hope of contributing something toward the solution of the broader question first stated, that, while working in the laboratory of Professor von Helmholtz at Berlin during the winter of 1883, I devised and constructed the apparatus presently to be described. In the mean time it is well to examine the physical grounds which render the correctness of the method a matter of greater or less probability.

In the first place it is evident that it is of fundamental importance for its success that the remaining constituents of the air should contribute nothing to the absorption produced by the CO_2 itself, and it is not difficult to show that the evidence afforded by the experiments above quoted is insufficient to warrant the conclusion that this is the case. Since the absorption of the whole depth of atmosphere above us, equivalent to 8 kilometers at the surface pressure, is but moderate (it is not necessary here to be very precise), it is evident that that produced by the length of a column such as we are able to make use of in our laboratories must be very small indeed, so small that we could hardly hope for success in detecting it by the usual direct method, the limits of error of our apparatus being much larger than the quantity to be detected, and this even in view of the fact that the absorption in the first layers of a gas is greater than in the subsequent ones. In the case of CO_2 however, we have a very different state of things. Here we can employ within reasonable limits an amount of gas equal to or comparable with that traversed by the sun's rays before reaching our position on the earth, and may expect to produce as great an effect.

The experiments of Dr. Heinrich Heine* are much more conclusive. By measuring with a delicate form of manometer the increase of pressure of the enclosed gas due to warming by the absorbed radiant heat from a Bunsen burner, he found that while perfectly dry pure air produced no change whatever, the admixture of ·025 per cent of CO_2 produced a readily measurable increase, so that he was able to make very accurate analyses of the air by comparing its effect (after drying) with that of mixtures of known proportions. He did not however experi-

* Ueber die Absorption der Wärme durch Gase, und eine darauf beruhende Methode zur Bestimmung des Kohlensäuregehaltes der atmosphärischen Luft. Giessen, 1882.

ment on the absorption of air containing aqueous vapor or other gases.

Further, the *character* of the absorption of CO_2 must agree with that of the air if this gas is the seat of the absorptive power of the latter, and this gives us an independent means of decision. The absorption of a substance which permits the passage of radiant energy will exhibit peculiarities of which the following constitute the most marked cases. It may be strongly selective, i. e. confined to certain definite wave-lengths, by which lines would be produced in the continuous spectrum of a luminous source whose rays had passed through the substance; it may affect strongly rays differing but little in wave-length, producing broad bands in the spectrum; or it may extend over many wave-lengths, affecting each but little more or less than the next in succession, by which no perceptible lines or bands would be produced, although the intensity of the spectrum would be weakened within wide and indefinite limits. The latter has been shown by Professor Langley* to be the case with the atmosphere, and a very important part of this general selective absorption is in the visible part of the spectrum, increasing in amount with decreasing wave-length until it becomes very powerful in the violet and ultra violet. Indeed, even without the table of coefficients of absorption furnished by Professor Langley, the difference in the appearance of the sun at noonday and at sunset suffices to show that a very considerable part of the action of the atmosphere is exercised upon the visible solar rays. Since sharp lines caused by water vapor are found in the spectrum (representing however in themselves a very small amount of energy) the total atmospheric absorption is a complication of at least two of the above classes.

Further, the labors of the same investigator show that the true value of the solar constant is considerably greater than that deduced by ordinary actinometric methods, so that there would remain a certain amount of absorption unaccounted for by the action of carbon dioxide, assuming the latter to have been correctly measured; finally, analyses have shown that the percentage of CO_2 in the air is tolerably constant in all parts of the earth, whereas the absorbing power of the atmosphere varies greatly.

The apparatus which I made for investigating the absorption of CO_2 was originally intended to be used in connection with a Rowland grating, so as to test the absorption of nearly homogeneous rays; but, owing to the limited amount of time at my disposal after it was completed, I was obliged to content myself with the use of artificial sources of heat, which, although far from furnishing homogeneous rays, differed so widely in character that the results obtained seem to me to be sufficiently

* This Journal, xxv, March, 1883.

definite to indicate the true character of the absorption exercised by carbon dioxide on the rays of the sun.

The tube containing the CO_2 in this apparatus was 18^{cm} in diameter and 1·7 meters long, made of tin-plate, and open at both ends. It was placed vertically on a low tripod stand provided with foot screws, the lower end dipping into a shallow annular trough filled with quicksilver, by which it was completely closed. Concentric with the trough, and likewise supported by the stand, was a silvered glass concave mirror of one meter focal length and 16^{cm} in diameter, forming as it were the bottom of the tube when the latter was in place. By this arrangement the tube could be readily removed, while when in place it was stopped in a perfectly air-tight manner at the lower end, and the direction of the axis of the mirror could be adjusted by the foot screws without any motion being communicated to the tube. The upper end was left open, and reached nearly to a wide flat board, firmly supported in a horizontal position by a suitable stand or scaffold, from which projected arms to steady and hold the upper end of the tube. In this board, perpendicularly above the center of the mirror, was a hole, much smaller than the tube itself, over which was a combination of two rectangular plane mirrors, likewise of silvered glass, joined at right angles like a letter V, with the lower angle over the middle of the hole and the reflecting surfaces facing outward, so that each made an angle of 45° with the vertical. The line joining their centers was therefore horizontal, and was adjusted to a height of 6^{cm} above the upper surface of the board. In front of one of these mirrors was a slit, consisting of a hole 1^{cm} square in a copper plate faced on the side next the mirror with pasteboard. Directly opposite this, in front of the other mirror, and at the same height, was a similar although smaller aperture exposing the central strips of a bolometer, which was held by a sliding carriage, so that its distance from the mirror could be altered at pleasure. If then a source of light was placed in front of the slit, the rays passing through fell first upon the nearer inclined mirror and were reflected down through the tube upon the large concave speculum, thence converging back through the tube upon the second inclined mirror, and brought to a focus in front of it, forming an image of the same size and at the same height above the board, as the slit, but lying on the opposite side of the two mirrors, so that the bolometer aperture could be brought into the image by sliding the movable carriage along its ways. Between the slit and its image the rays had to pass through a distance equal to four times the focal length of the concave mirror or 4 meters, 3·4 meters of the distance being traversed within the tube. To facilitate the adjustments the slit was

made movable in its own plane through a small distance in every direction, and all possibility of direct rays from it reaching the image was removed by interposing a series of screens.

The bolometer used, for which I am indebted to the kindness of Prof. Langley, had a resistance on each side of about 30 ohms, and exposed a surface of 26 $^{sq.mm}$. It was fastened upon a small hinged lid, which, when shut, brought the working surface of the bolometer exactly opposite an aperture of the same size in the copper plate that formed the front surface of the sliding carriage, the bolometer in this position being otherwise completely enclosed in a copper lined cavity. The galvanometer used in connection with the bolometer was a very small instrument on Sir Wm. Thomson's plan with modifications by Prof. von Helmholtz. The mirror, on the back of which were cemented two minute bar magnets, was suspended by a single spider's thread 6^{cm} long, so that there was no perceptible torsion. The deflections were observed by the telescope and scale method, and, as they never exceeded 10^{cm} on the 126^{cm} distant scale, they required no reduction. Under the influence of the earth's magnetism the mirror made a complete double swing in 1·5 sec., but was rendered so nearly astatic by means of a bar magnet below the galvanometer pier, that this time was increased to 15·0 sec., or, when a lamp was used as source of heat and the apparatus had to be rendered less sensitive, only to 8·4 sec. The galvanometer had a resistance of 173 ohms, and, although by no means specially adapted to the purpose, its performance in connection with the bolometer was in every way satisfactory. The current used was produced by two small Daniell's cells, and its strength, as indicated by another galvanometer, was so constant as to render corrections for change of current unnecessary.

The lamp, or other source of heat, was placed in front of the slit upon a shelf provided for the purpose, and all radiation except that passing through the slit was cut off by a large screen faced with bright tin plate, which formed the front of the apparatus. The exposure was made by means of a sliding shutter in front of the slit, which was opened by the observer at the galvanometer, when required, by pulling a cord. Further, in order to cut off extraneous radiation and air currents, the whole of the apparatus on the board was covered by a box of tin plate, and the upper end of the tube surrounded by protecting screens.

The manner in which this apparatus was used is apparent. The bolometer having been "balanced" and the mirrors and slit adjusted so that the image of the latter fell upon the bolometer aperture, heat from the source experimented upon was admitted by pulling the cord, and the galvanometer gave a prompt

deflection which was noted by the observer. The tube being then filled with carbonic acid, which on account of its specific gravity could not escape, a smaller deflection was obtained, indicating a certain absorption in a column of gas double the length of the tube. It remains to describe the way in which the filling was accomplished.

A short glass tube passing up by the mirror through the bottom of the low stand could be connected at pleasure, by means of a length of clean rubber tubing, either with the gas generator or with a water air-pump in an adjoining room. The generator consisted of a bomb containing eight kilograms of pure liquid CO_2, from which a practically inexhaustible supply of gas could be drawn and the tube filled in as short a time as desired. By disconnecting the generator and connecting the air-pump the gas was withdrawn from the tube and ejected from the room.

Such being the nature and arrangement of the apparatus employed, the manner of conducting an experiment was as follows: The doors and windows were opened half an hour before the beginning of operations, and the air-pump connected and set going, in order to fill the room and tube with pure air. The room was then closed, the pump stopped, and a series of exposures made at noted intervals of about $1\frac{1}{2}$ minutes for half an hour or more, in order to form an idea of the constancy of the lamp (or other source), and obtain the rate of change of its intensity. Then, without disturbing the apparatus, the tube was connected with the reservoir and filled with CO_2, the moment of overflow being tested by means of a spark on the end of a wooden splinter, for which purpose a small offset tube had been provided, branching off from the large one near the top. In order to prevent air currents from disturbing the level of the gas, the mouth of the large tube was covered during the operation by a sheet of paper. When full, which was generally four or five minutes after connecting with the reservoir, the gas was nearly shut off, a very slight current being allowed to flow to supply the loss by diffusion, and several observations made as before with the galvanometer. These deflections were, in the case of the lamp, somewhat smaller than the first. The tube was then connected with the pump and the observations continued, the deflections increasing as the level of the CO_2 sank in the tube, and regaining their original value when the gas was all out.

As an example, I give the record of observations made on the 15th of March, the source of heat being in this instance the non-luminous flame of a Bunsen burner.

Time.	Deflec. in mm.	Time.	Deflec. in mm.	Time.	Deflec. in mm.
$4^h\ 27^m$	82·4	$4^h\ 52{\cdot}5^m$	Filled with CO_2.	$5^h\ 18^m$	56·3
28	82·4	56·5	50·1	19·5	59·3
29	81·1	57·5	51·2	20·5	61·5
30	80·9	58·2	50·4	21	Gas nearly out.
31	81·4	59	51·0	22	63·3
33	79·8	5 1	50·4	22·5	appar'tly all out.
34	79·9	1·5	connected pump	23	69·8
36·5	79·7	6	50·4	25	73·0
39	81·0	7	50·1	31	73·0
40	78·7	12	51·6	32	72·9
48·5	80·3	13	52·2	34	73·5
50	80·2	14	52·9	39	72·8
51	78·2	16	54·5	40	74·0
52	78·4	17	54·6	41	73·5

If these observations be plotted on paper with abscissæ proportional to the time and ordinates proportional to deflections, we obtain a curve showing graphically the absorption of the rays from the burner at different times, and hence approximately for different thicknesses of CO_2. From this curve we take at time $4^h\ 57{\cdot}5^m$ deflection with air in tube $78{\cdot}1^{mm}$; deflection with CO_2 in tube $50{\cdot}4^{mm}$ reduced to time at $4^h\ 57{\cdot}5^m$.

This gives an absorption of 35·5 per cent. From a consideration of all the observations an absorption of 35·8 per cent was taken as that due to the tube when full.

Let us now consider the absorptive media interposing between the source of radiation and the bolometer face before the admission of CO_2. We may disregard the glass chimney of the kerosene lamp, since we may regard the whole combination as forming a single source of heat, remarking, however, that as the glass is heated, and consequently radiates the same rays it absorbs, its absorptive effect is not so great as if it were placed elsewhere in the path of the rays. Between the slit and the bolometer we have then four meters of air and three reflecting surfaces of silver. We have seen that the absolute absorption of such a column of air must be very small, although doubtless many times that of such a vacuum as can be produced by an air-pump. The amount of CO_2 passed through in this distance is from 1 to 2 millimeters. The silver surfaces have been shown to exercise a general selective absorption, increasing gradually from the lower to the upper end of the spectrum, but nowhere exhibiting sudden changes. When the tube is full of gas the conditions are exactly the same, except that 3·40 meters of the path of the rays lie in CO_2 instead of air. The proportion absorbed by the silver is almost precisely the same as before, that of the air may be neglected in either case, and we may therefore take the ratio of the deflections obtained

under the two different circumstances as the proportion of the total radiation transmitted by 3·4 meters of gas.

The first source of heat employed was a kerosene lamp with large argand burner and glass chimney, and was more constant than any of the sources tried later, the irregularities in a series of observations being only about one-half per cent of the average value of a deflection. Three experiments gave an absorption of 7·1, 7·2 and 7·9 per cent, the mean of which is 7·4 per cent.

The next source was a copper plate heated from behind to a point just below redness by a Bunsen flame, so that all of the radiations consisted of invisible rays. Of these 11 per cent were absorbed.

Next was tried the feebly luminous flame of the Bunsen burner itself. This indicated a greater absorption than any of the other sources, three experiments giving a mean of 35·8 per cent.

Next was tried a source consisting as much as possible of luminous rays only. For this purpose I employed the lamp, placing against the slit on the inside a water-cell with thin glass sides, containing a thickness of $15{\cdot}3^{mm}$ water. This cell cut off 90 per cent of the radiation from the lamp without perceptibly diminishing the brightness of the image on the bolometer, and a prism placed behind the bolometer aperture showed a spectrum extending well up into the violet. No absorption whatever was indicated in this experiment, which was repeated under various conditions of the apparatus. The water cell completely cut off the rays from the Bunsen burner.

Finally, although the apparatus did not admit of experiments with aqueous vapor, the enormous absorbing power of water was strikingly shown by a soap-bubble film stretched on a wire loop, which cut off 38 per cent of the rays from the Bunsen burner, or more than the 3·4 meters of CO_2. The soap solution from which the film was obtained, when placed in the water cell, indicated an absorption of the rays from the lamp 20 per cent greater than that of water.

In these results we may especially note the following facts: 1. That the luminous rays of the spectrum are not appreciably absorbed by a column of CO_2 3·4 meters in length, whereas it has been shown that these rays are strongly absorbed by an atmospheric column containing the same quantity of CO_2. The evidence afforded by the eye, inasmuch as the gas appears perfectly transparent, is confirmed by direct measurement of the energy of transmitted luminous rays. 2. That as the range of the wave-lengths given out by the radiant source is narrowed down toward a certain invisible limit, the absorption of the gas becomes greater and greater, corresponding to the production

of an absorption band in the ultra red of a continuous spectrum. 3. That the absorption is very considerable, and hence cannot correspond to a narrow line, but rather to a broad and heavy band.

I therefore consider it most probable that to the action of CO_2 in the atmosphere is due one or more of the great gaps in the invisible part of the solar spectrum, which the discoveries of Professor Langley* prove to be much more extensive than had hitherto been supposed, but further regard it as certain that some other agent than this gas (which may not be water vapor, or possibly even of a gaseous nature), contributes essentially to the total absorptive power of the atmosphere, so that a method of analysis based upon this power, in which the effect of the second agent is neglected, cannot lead to correct results.

The strongly selective absorption of CO_2 is also shown by a single series of observations, such as that given at length above. The deflections of the galvanometer rise very gradually as the level of the gas sinks in the tube, being very little greater than at first when it has fallen half way, then increase rapidly as the last portions of the pure dense gas are pumped out, and continue to rise slowly for some time after. The latter rise is caused by the gas, which, during the time occupied in pumping out has become diffused upward through the tube, so that the air within contains a much greater percentage of CO_2 than before the beginning of the experiment, and the removal of which requires considerable time. This is best seen in the graphical construction, where these changes are represented by corresponding inflections in the curve. The strong absorption of the rays from the Bunsen flame is in accordance with what Kirchhoff's law would lead us to expect, since this flame consists principally of glowing CO_2 and H_2O. The absorption of rays from a source consisting entirely of heated CO_2 is still more marked, as was shown by Tyndall in one of his experiments, where one meter of CO_2 almost completely cut off the rays from a flame produced by burning carbon monoxide, CO, in the air. Thus the absorption spectrum of CO_2 would be the reverse of the spectrum of such a flame.

Berlin, April, 1884.

* This Journal, xxv, March, 1883.

NOTE.—Since the above was written I have seen in Professor Langley's article on the determination of obscure wave-lengths (American Journal, vol. xxxvii, March, 1884), that observations made at his observatory make it probable that the largest of the gaps referred to are of telluric origin.

Art. XXV.—*Triassic Insects from the Rocky Mountains;* by Samuel H. Scudder.

Early in 1882, Mr. Arthur Lakes, Professor in the Colorado School of Mines, discovered a bed of plants and insects near Fairplay, Colorado, in rocks much older than any that have before yielded insect remains west of the Great Plains; the two or three specimens he sent me were sufficient to prompt a more thorough exploration of the locality, which I was able to make the following summer, resulting in the discovery of a fauna and a flora of considerable interest.

The plants have been studied by Mr. Lesquereux,* who pronounces the species, some thirty in number, but in a very fragmentary condition, to belong to Permian types, and declares the evidence to be decisive on this point.

The animal remains consist almost exclusively of insects, and are two-thirds as abundant in species as the plants—an exceptionally large ratio in beds where both occur. These insects form an assemblage wholly different from anything before known and, in contradiction to what Mr. Lesquereux says of the plants, clearly belong to types of a more modern character than any the Paleozoic series has yet disclosed. It is not often that one may speak so positively in the discussion of fossil insects, especially when not a single one of the species and only the smaller portion of the genera found have been previously known. But in this case all but two or three of the specimens obtained (some eighty in number) belong to a group which of all Paleozoic insects has received the most attention, namely, the cockroaches. This great preponderance of cockroaches, and the fact that the few known genera found in this collection have hitherto been discovered only in Carboniferous and Permian rocks, would lead us at first to refer the beds in which they occur to one of the Paleozoic series; but the presence of the other forms, and even the characteristics of those which are referable to Carboniferous and Permian genera, unmistakably point to a later horizon.

Paleozoic cockroaches are distinguished from living types by the complete interdependence of two of the veins of the front wing, and by the fact that the anal veins of the same wings invariably impinge upon the inner margin, and never, as in existing forms, upon the anal furrow. For these ancient types the name of Palæoblattariæ has been proposed, and all Paleozoic cockroaches whose front wings are preserved (and we know them almost exclusively from these organs) fall into this

* On some specimens of Permian fossil plants from Colorado. Bull. Mus. Comp. Zool., vii, 243.

group. So far as I can discover there is not a single exception to this difference between ancient and modern types. Since this was first stated five years ago the number of Paleozoic species has been increased 25 per cent, and it is still true.

In the paper in which these points were first discussed no allusion was made to Mesozoic cockroaches, as none had been found in this country, and the illustrations we possess of the European species are in many cases by no means sufficient to expose their structure; their study was therefore left until the imperfection could be remedied. It was however recognized, though not stated, that Palæoblattariæ exist in Jurassic rocks; it is shown, for instance, by figures of Wealden species on the fifth plate of Brodie's work on the "Fossil insects of the secondary rocks of England" (London, 1845), and by Dr. Eugen Geinitz in his recent paper on the Dobbertin insects,* in which one species is figured from the lower Jura; but the great mass of Jurassic species are plainly more closely related to living forms, and neither in the independent existence of the veins which are characteristically distinct in Paleozoic types, nor in the course of the anal nervules, do they show any affinity to the Palæoblattariæ.

Eleven of the seventeen species of cockroaches, and five of the nine genera found at Fairplay belong to the Palæoblattariæ. These five genera are the following: Etoblattina (1 sp.), Petrablattina (2 sp.), Anthracoblattina—very doubtful, the specimen being very imperfect—(1 sp.), Spiloblattina, nov. gen. (4 sp.) and Poroblattina, nov. gen. (3 sp.). Only four of the eleven species therefore belong to known genera and one of these is doubtful; but the difference is more marked than this; for the species referred to Etoblattina is an aberrant form with an excessively long internomedian vein; and both the species of Petrablattina agree in differing from those heretofore known to a very considerable degree. Of the new genera, Spiloblattina is very peculiar in the strongly divergent and then convergent curve of the externomedian and internomedian veins around a large stigma near the middle of the wing, unknown in any other cockroach, ancient or modern, so far as I know; but otherwise it is related to Etoblattina; while Poroblattina is more nearly related to Petrablattina, and especially to the two new species of that genus from this locality.

The average size of these Fairplay Palæoblattariæ is much less than that of the Paleozoic Palæoblattariæ in general. The average length of the front wings of the Paleozoic species is 26^{mm}; that of these Fairplay Palæoblattariæ, 16^{mm}. This fact has its value, for the Jurassic species are nearly all of very small size, and the wing-length of the remaining species from

*Zeitschr. deutsch. geol. Gesellsch., 1880, p. 510.

Fairplay (i. e. those which do not belong to the Palæoblattariæ) is less than 8·5, ranging from 6·5–11·5^{mm}. This agrees completely with the size of Mesozoic species already known. The average of all the Fairplay cockroaches is less than 13·5^{mm}.

As to the six cockroaches from Fairplay which do not belong to the Palæoblattariæ, the characteristics of their venation, as well as their small size, show them to be closely allied to Jurassic forms, although the three or four genera to which they belong are distinct from any yet characterized. Two of them are distinctly allied to Rithma, a genus established rather loosely by Geibel for some species from the English Purbecks figured by Westwood. They all have a decided Mesozoic aspect, and would at once be considered Liassic or at least Jurassic by any one familiar with the forms already known from these deposits. They have on the other hand an entirely different aspect from any and all Paleozoic forms, and present no points of close comparison with any Palæoblattariæ excepting some of those mentioned above from the same Fairplay beds, notably with the genus mentioned under the name of Poroblattina, which one of the genera not a little resembles.

This resemblance is of special interest because it points out the method in which the change from Paleozoic to Mesozoic forms has taken place, and does not bear out the suggestion made in my memoir on Paleozoic cockroaches (based on a comparison of the venation of the front and hind wings of existing cockroaches), that the scapular and externomedian were the two veins which were amalgamated in the historical development of the group. For when we compare the series of genera near the boundary line of the departure of the Palæoblattariæ toward later forms (those Paleozoic cockroaches allied to Petrablattina) and especially those brought to light by the discoveries at Fairplay, we find that in the Mesozoic species at least, it is the mediastinal and not the externomedian vein which has blended with the scapular, although the externomedian also may become blended with the others in living types. This amalgamation has proceeded by the enlargement of the scapular area, which has crowded the mediastinal toward the base of the wing, whose few remaining branches finally become attached to the scapular vein, no trace of their former dependence remaining visible.

We have then at Fairplay an assemblage of forms altogether different from any thing hitherto found in the Paleozoic series on the one hand, or in the Jurassic beds on the other. They show a commingling of strictly Jurassic forms with a larger proportion of types which may be called Upper Carboniferous or Permian with a distinct Jurassic leaning. There is therefore a strong probability that the beds in which they occur belong to the intermediate formation, the Triassic.

If this should be proved, Mr. Lake's discovery will have an added interest, from the fact that almost nothing is known either of the plants or of the insects of this formation. Of the plants, it is only necessary to point out that in the paucity of data, the upper Paleozoic aspect of the few vegetable remains from Fairplay can have but a negative value beside the positive proof of the alliance of the insects to Mesozoic forms. Of Triassic insects our knowledge is exceedingly meager; a single neuropterous larva from the Connecticut valley is all that the formation has hitherto yielded in this country. In Europe we know of only four species, each, I believe, from a single specimen; one of these is a cockroach, but it is entirely different from any of the Fairplay species, and indeed from any other known forms, so that we get no light from this quarter.

It may be urged that as much the larger proportion of known Paleozoic cockroaches come from Europe, our own fauna being comparatively unworked, this discovery may only indicate for America an earlier advance within Paleozoic times toward later types. Besides the important consideration that this would be in direct opposition to what we know of subsequent periods in America, there are only two facts known to me among fossil insects bearing on this point, one in favor of this hypothesis, the other against it. The first is the recent discovery in beds at Kansas City, Mo., said by the state geologists to have eight hundred feet of Carboniferous rocks above them, of the wing of a heteropterous Hemipteron, which I have called Phthanocoris. In Europe no instance is recorded of any insect belonging to this great group of Hemiptera in Paleozoic rocks, the three or four Hemiptera so far found belonging to the homopterous division. The other fact is brought forward in my memoir on Paleozoic cockroaches, and is of far more importance, not only because it is of broader significance, but also because it is drawn from the same group as that under discussion. The Palæoblattariæ are divisible into two groups, the Mylacridæ and the Blattinariæ, the former of which is in point of structure the more primitive type. Now the Mylacridæ occur only in America and form indeed about two-thirds of the species known from this continent. In Carboniferous times, therefore, as regards cockroaches, America was *more* old fashioned than Europe, and we should look for the introduction of new elements earlier in Europe than in America; yet the better explored Carboniferous and Permian deposits of that continent have yielded no traces of anything akin to the Fairplay insects. The first appearance of any such is in Mesozoic strata, and notably in the Lias.

So far as I know this is the first attempt to determine the age of a deposit from its insect remains alone, and it is unfortunate for its acceptance by naturalists that the plants give it, to say

the least, no support, but rather are deemed by one competent to judge, to be decidedly adverse to what is here claimed.

The paleontological contradiction shown in the plants and animals of the Fairplay beds is not unknown to American geology, as every one is aware, but I do not know that it has been pointed out in this country at this horizon or in this direction—the discordance appearing later in time, and the plants indicating a younger and not an earlier age than the animals. An exactly parallel case appears to be shown in Eastern Russia, for in discussing the poorer strata of Kargalinsk, which he refers to the Permian, Twelvetrees says, "as regards the flora [11 species] the list has a Paleozoic aspect, but a secondary one as respects the reptilian remains" [4 species cited].*

Exploration of the locality will continue, and it is hoped that future material may throw more light upon the question. It may, however, be added that the few other insects found appear to have no Paleozoic relations whatever.

ART. XXVI.—*On the Flexibility of Itacolumite;* by ORVILLE A. DERBY.

HAVING had many occasions for observing the extensive series of quartzose rocks known as itacolumite in the gold and diamond regions of Minas Geraes, Brazil, I have for some time entertained a suspicion that the peculiar property of flexibility would on investigation be found to be only a surface character. I have already shown (this Journal, vol. xxvi, p. 34) that much of the so-called itacolumite belongs to a higher geological horizon than that containing the flexible layers and that in the mountain of Itacolumi itself only a small part of the mass of the mountain is formed of the schistose beds of the lower series of quartzites in which alone, so far as I am aware, the property of flexibility has been observed. Even in the lower series flexible portions are rather uncommon, and the greater part of the mass is as rigid as any similar rocks.

Artificial openings into this series of rocks are extremely rare, and so far itacolumite has been studied in its natural outcrops only at the surface, in which it is generally impossible to determine exactly how much the original characteristics of the rock have been modified by weathering. For some time past I have been on the lookout for an opportunity for studying unweathered itacolumite in order to determine whether or not the flexible layers extend beyond the action of surface agencies. Such an opportunity was recently afforded by the newly constructed Rio and Minas railroad. This line, starting

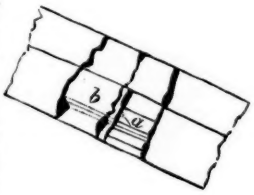
* Quart. Journ. Geol. Soc. Lond., xxxviii, 495.

from the valley of the Parahyba, crosses the Mantiqueira division of the maritime range of mountains into the southern part of the province of Minas Geraes, and then follows the course of the Rio Verde, one of the headwaters of the Rio Grande branch of the Parana, in the mountainous plateau lying to the westward of the Mantiqueira. The cuttings are mainly through gneiss, in great part decomposed, but at several points narrow belts of itacolumite are also cut through. At a place called Jurumirim, the river and railroad pass by a short, narrow gorge through a high ridge of itacolumite extending from S.W. to N.E. In the sides of this gorge the rock is unusually well preserved and has been cut away to give passage to the railroad to within two or three meters of the drainage level. A few miles away to the northeast on the same ridge is the village of São Thomé das Lettras, famous for itacolumite quarries which have afforded beautiful flexible specimens. This locality is about fifty miles southwest of São João D'el Rei, a typical itacolumite locality, from which this formation has been traced continuously to and beyond the Serra de Itacolumi at Ouro Preto. Though the intervening region has never been examined, there can be little doubt that the beds here examined are identical and continuous with those of São João D'el Rei and Ouro Preto.

This cutting exposed very clearly the rock in its natural state and as affected by percolating waters and surface agencies. The total thickness of the itacolumite series in the range of hills is not far from 100 meters, of which about 40 meters near the center of the series are laid bare in the cutting. Of these 40 meters the upper portion is composed of massive beds from one to three meters in thickness, in which flexible portions are rarely found. In the lower part the beds are more schistose or flaggy, and divide readily into laminæ of a few millimeters to a few centimeters in thickness, the greater part of which are more or less flexible. In this portion, water percolates freely along the planes of stratification and lamination as well as along the numerous joints, and it is difficult to find masses that can with certainty be pronounced to be in their original state. The beds strike N. 60° E., and dip to the N.W. at an angle of 30°.

Among the heavy upper beds the action of the weather and of percolating waters has been less general and an opportunity of comparing weathered and unweathered portions is afforded. The beds are traversed by fissures, and in the masses separated by fissures and stratification planes, massive and schistose, compact and friable, non-flexible and flexible portions may be found within a distance of a few centimeters, and in exactly the same relative position in the bed. Several such masses were observed, of which one is represented in the following figure.

The block, *b a*, about two meters thick and separated by open fissures from its neighbors on either side, is traversed near the middle by three cracks. From between two of these cracks a prism-shaped mass had fallen out, leaving an open space. Water percolating through these cracks has opened several concealed planes of lamination in the right-hand portion (*a*); while to the left of the cracks, although these planes are clearly indicated by faint lines of color, the stone shows no signs of yielding along them, and fragments detached with the hammer at *b* do not break along the dotted lines more readily than in any other direction. At *b* the rock is exceedingly hard and not at all friable or flexible, while at *a* it divides readily into slabs two or three centimeters thick, crumbles to sand with the pressure of the fingers and is slightly flexible. It should be stated, however, that in preparing a microscopic slide from the rock at *b* the thin slice showed a considerable degree of flexibility, though perhaps not more than other granular rocks which are not at all flexible in masses may exhibit under the same circumstances. My experience in making slides is not sufficient to enable me to form an opinion on this point.



The property of flexibility is best seen in the thin plates into which the rock splits along the planes of lamination, that is to say, in the direction of the bedding. Owing to the difficulty of detaching pieces of suitable shape it is not so readily exhibited in other directions. At several points, however, where thin slabs or irregular prisms had been separated by fissures transverse to the bedding, it was also observed. The planes of stratification and lamination are thickly covered with silver-white mica, but in the massive portions of the rock this mineral is not at all prominent.

From these observations it may be concluded that flexibility is not an original characteristic of the rock, but that, like the opening of the planes of lamination and the development of friability in hard granular rocks, which it accompanies, it is a phase of weathering or of the decay brought about by percolating waters. So far as known, it is limited in Brazil to beds of a certain definite geological horizon, apparently of Huronian age. It would appear, however, that it might occur equally well in any fine-grained and thin-bedded or laminated quartzite or ordinary sandstone. Further examination will very probably show that it is not limited to rocks of any definite horizon or to any well characterized natural division of the quartzose rocks.

ART. XXVII.—*On the Age of the Glazed and Contorted Slaty Rocks in the vicinity of Schodack Landing, Rensselaer County, N. Y.;* by S. W. FORD.

DURING the latter part of the month of May, after a day spent in the study of the Primordial rocks of the town of Stuyvesant, Columbia County, the writer made a hasty examination of the folded slates directly back of the Schodack Landing railroad station, near the Hudson River, and collected therefrom two poorly preserved, but yet decisive specimens of the well-known graptolitic species, *G. pristis.* It occurred to me at the time that the locality was one deserving more extended investigation; but owing to the pressure of other engagements, I was then prevented from entering upon the work. I have since, however, found opportunity to devote considerable time to the exploration of the region, and some of the more important results obtained are herewith communicated.

The slaty rocks back of Schodack Landing station, and their associated beds, occupy all the higher ground to the south as far as the Columbia County line, and are even prolonged a short distance into the town of Stuyvesant in Columbia County. In passing southward through Schodack Landing, they sweep westward as far as the Hudson River railroad track in two conspicuous promontories, and finally disappear altogether, about a hundred yards distant from the Stuyvesant Primordial beds,* a *fault* intervening. In these promontories the slates are excellently exposed, and are bent and contorted almost beyond description. They are fossiliferous at several different points, but especially so along the line of the Hudson River railroad, a short distance south of the county line, and about two hundred yards north of the residence of Mr. Patrick McCabe. At this locality there occurs a band of black slates, but slightly altered, which has thus far yielded the following ten species of Graptolites, described and figured on pages 265–274, plates 72–74 of the first volume of the Paleontology of New York, besides two others, probably new or undescribed:

G. pristis,	*G. ramosus,*
G. mucronatus,	*G. scalaris,*
G. sagittarius,	*G. sextans,*
G. tenuis,	*G. furcatus,*
G. bicornis,	*G. gracilis.*

* Directly south of the more southerly promontory, the surface is low or marshy; and, after crossing this, the rocks at one point resemble those of the Hudson River group, although beds clearly of Primordial age occur only a few yards distant. It is possible, therefore, that the two groups have come into contact with each other, but I have not, as yet, been able to fully satisfy myself upon this point.

In addition to the above, I have also obtained from this band the young graptolitic forms figured on plate B of Decade II of the Geological Survey of Canada (1865), figs. 15 and 18. In view of all the evidence, I think there can be no doubt that these slates are the exact equivalents of the Graptolite-bearing beds of the Norman's Kill, near Albany. Stipes of the *G. sagittarius* have been obtained here over a foot in length, and all of the species occur in a remarkably perfect state of preservation.*

The Norman's Kill slates were assigned by the New York Geological Survey to the horizon of the Hudson River group; and although doubts were subsequently raised by the investigations of the Canadian geologists as to the correctness of this reference, I became satisfied in 1871 and 1872, after an examination of the slates of various other localities on the west side of the Hudson River, that it was, in all probability, the right one. Nevertheless, so long as the graptolitic testimony of the Norman's Kill beds remained unsupported by the evidence of other Hudson River group fossils at that locality, caution appeared to me necessary in pronouncing upon the age of beds characterized by simply one or more of these species of graptolites, and not demonstrably their stratigraphical equivalents, in other places. For instance: in the summer of 1870, I obtained specimens of *Graptolithus pristis*, *G. scalaris*, and a third species which I was unable to determine, from the slates immediately east of the Hudson River at Troy; and later I found good specimens of the first mentioned species in the folded slates between Troy and Lansingburgh, along the line of the Troy and Boston railroad; but I did not consider this as demonstrative evidence of the Hudson River age of the beds, although I now believe them to be of this age, in view of the new facts that have recently come under my observation.

With the discovery of the rich graptolitic locality in the vicinity of Schodack Landing, I at once resolved to institute a careful search for other fossils in the rocks of the neighborhood; and before quitting the field, my researches were rewarded with success. Directly back of Mr. Honstein's blacksmith shop, at Schodack village, there is a bed of limestone, about two feet thick, and, in part, somewhat brecciated in appearance, enclosed in the slates; and about a quarter of a mile from the river, in the bottom of a deep gorge running eastward along the Columbia County line, the same limestone band, similarly situated, is again met with. From the mode of

* The same or a similar band, abounding in the same species, occurs about a mile south of Castleton, on an east-and-west road connecting with the regular highway from Schodack Landing to that village. It strikes obliquely across the former highway only a few rods from the latter, or just beyond the residence of Mr. Collins.

occurrence of this band, I have no doubt that it is a regular member of the slate formation. It is fossiliferous at both of the points referred to, and has thus far furnished the following species: *Asaphus platycephalus*, *Calymene senaria*, *Orthis testudinaria*, *Orthis lynx*, *Leptæna sericea*, *Strophomena alternata* and the hemispherical variety of *Chætetes lycoperdon*. None of the species of this locality are distinctive of the Utica slate, and both the limestone and its associated graptolitic slates represent, in my estimation, the Hudson River group. It is possible, however, that the Utica formation will yet be somewhere recognized among the folded and contorted rocks of the region.

In view of what I have recently and for several years past, observed in the field, the following propositions appear to me to rest upon a good strong foundation of facts, and will, I have little doubt, be fully sustained by future research:

1. That the rocks of the Hudson River group skirt the Hudson River quite extensively upon the east in Washington, Rensselaer, Columbia and Dutchess counties, or, at any rate, extensively enough to entitle them to continue to be called the Hudson River group; although the better plan would appear to be to discard altogether that designation, and go back to the old term "Lorraine shales."

2. That a great dislocation, bringing up (at least for a large portion of the distance) the rocks of the Primordial zone upon the east side of the line of fracture, and placing them upon the same level with, or even higher than, the rocks of the Hudson River group upon its western side, runs from western Vermont southward through the western portion of Washington, Rensselaer, Columbia and Dutchess counties; and that this dislocation does not strike across the Hudson River opposite, or so as to pass through Rondout, as some have supposed; but, on the contrary, before crossing, extends on southward beyond Rhinecliff station, in Dutchess county, for several miles.

3. That this dislocation probably occurred at the close of the Lower Silurian as urged by Professor Dana.

4. That over a considerable part of the territory traversed by this fault, the Primordial rocks wall against those of the Hudson River group unconformably; but that, in the majority of instances where they come together, the older beds have been made to overlap the newer conformably.

Johnstown, N. Y., July 5th, 1884.

ART. XXVIII.—*The Relations of the Mineral Belts of the Pacific Slope to the Great Upheavals*;* by GEO. F. BECKER.

PROFESSOR W. P. BLAKE first drew attention to the series of mineral belts of the Pacific slope and their striking parallelism to the mountain ranges, enumerating a considerable number of more or less continuous lines of ore deposits and stating their relations to the mountain system.† Mr. Clarence King accepted and enlarged upon these facts.‡ All the structural features of local geology, he says, including the distribution of the precious metals are grouped in strict subordination to the longitudinal direction of the ranges, and these represent folds parallel to the main axis of the whole system of elevation. The deposits themselves he ascribes to the solfataric action accompanying the ejection of igneous rocks, and these eruptions he connects with the great orographical disturbances of the Jurassic and Tertiary eras.

It is fourteen years since the well known volume containing these statements appeared, and in the mean time great additions have been made both to the number of known ore deposits on the Pacific slope and to our knowledge of the geology of the region; contributions which throw some additional light on the relations between the broader facts of structure, and the distribution of ores. In most respects the statements made by Mr. King have been confirmed by subsequent information. The close relations between the folds represented by the ranges of the Pacific slope and the occurrence of ores is placed beyond question. Neither the ranges nor the deposits accompanying them however, have proved on the whole so continuous as it was once believed they might do. The mountain system of the Great Basin and Arizona may properly be regarded as a vast chain; the individual ranges being frequently interrupted and represented, but not continued in the direction of their trend, by fresh ranges. Similarly, while there is an evident tendency in almost every mining region west of the Rocky Mountains to prolongation in the direction of the trend of the mountains, comparatively few of the series of deposits are sufficiently continuous to be classed as mineral belts. Indeed it would now scarcely be claimed that there are more than four distinct ore belts on the Pacific slope, viz: the lead silver belt of Utah, the gold belt and the quicksilver belt of California,

* A part of the substance of this paper was first written for a Census report on The Statistics and Technology of Precious Metals, by Mr. S. F. Emmons and myself, now in press.

† Report to the California State Board of Agriculture, 1866.

‡ Exploration of the 40th Parallel, vol. iii, Chap. 1.

and the chain of deposits which extend diagonally across, Arizona.

In the Coast Ranges of California, quicksilver and chromic iron occur at a great number of localities. The most southerly quicksilver mine known to me is Las Prietas, four miles from Santa Barbara, and the most northerly the New Idrian, Douglass county, Oregon, localities separated by an interval of over 600 miles. There are considerable differences in the width of the strip of country embracing the known quicksilver deposits which are very numerous in the latitude of Mt. St. Helena and extend for a distance of about fifty miles in a northeast and southwest direction, or at right angles to the general trend of the belt, while to the northward of Clear Lake the deposits are few and are separated by long intervals. On the whole, however, it is strikingly apparent that conditions favoring the deposition of quicksilver ores have prevailed at numerous points within a belt in most places of small width and which is of great length. Messrs. Whitney and Gabb, as is well known, ascribe the elevation of the Coast Ranges of California to a post-Miocene uplift. The western edge of the area raised, or the present coast of California, is nearly parallel with, and for the most part very close to, the quicksilver belt.

The gold belt of California is extremely well defined from the southern boundary of Mariposa county to the neighborhood of Nevada City, a distance of about 150 miles, and near it is a series of copper deposits scarcely distant enough to be regarded as a separate system from a general point of view. Gold deposits are found in great abundance but more scattered to the northwest of Nevada county, and there is also a little gold to the south at a number of points along the foothills of the Sierra to its termination at Fort Téjon; in fact though the gold belt proper is comparatively short, there can be no doubt that a gold-bearing zone of country lies along the western foot of the Sierra for several hundred miles. This zone also coincides with the western edge of the great area of Mesozoic rocks which, as was established by the investigations of the California State Survey and the Exploration of the Fortieth Parallel, was uplifted after the close of the Jurassic.

The Utah ore belt lies at the western base of the Wahsatch range and its southwesterly continuation. With the exception of the Leeds (Silver Reef) district, all the important deposits of the territory are included in this belt, which bears a very definite relation to the main line of crests. The Wahsatch also forms the western edge of the Rocky Mountain area, which was uplifted at the close of the Cretaceous; the famous Wahsatch fault-line passing through or close to many of the mining districts.

The Arizona deposits lie in a zone stretching entirely across the territory from southeast to northwest. This zone includes or lies close to the dividing line between Paleozoic strata to the northeast and the Archæan area of Southwest Arizona, deposits occurring both in the granites or crystalline schists and in the Paleozoic limestones. The main contact between the Paleozoic and the underlying strata is laid down, in the geological maps of the Surveys West of the 100th Meridian, continuously from Virgin Cañon to Camp Verde, a distance of 170 miles. Farther south the most westerly occurrences of Paleozoic shown are in the Pinal mining district near Florence and in latitude 32° 20′, longitude 109° 40′. These are probably near the edge of the area, though there is some evidence of detached patches still farther to the south, and to the west of the general course of the contact so far as traced. The Chiricahui range has been shown by Mr. Gilbert to be largely made up of Paleozoic strata, and the mines of the Tombstone district are many of them sunk on deposits in limestone. In this region limestones can hardly be other than Paleozoic, and they are reported as containing Carboniferous fossils.

The rocks adjoining the Paleozoic to the southwest are unquestionably Archæan, for their relations to the Silurian are clear at a great number of points, and their lithological character in this region is very characteristic and persistent. The edge of the Paleozoic has also been followed by Mr. Gilbert in a westerly direction into California, near Owens Lake, whence its course is deflected to the north (Mesozoic strata, in part overlying Archæan, coming in on the west) and it crosses the fortieth parallel in longitude 117° 30′. The mines of Battle Mountain, Austin, Candelaria, Cerro Gordo and Panamint all lie close to the contact. Scarcely anything is known of the geology of the region north of Battle Mountain, but Mr. Meek determined fossils collected on the northern boundary of the United States at the 114th Meridian by Mr. Geo. Gibbs as Carboniferous,* and it is probable that the contact passes through the mining region of Idaho.

The western edge of the Paleozoic in the belt of country surveyed by the Exploration of the Fortieth Parallel represents the structural line along which the Paleozoic area of Eastern Nevada and Western Utah was uplifted at the close of the Carboniferous. In Arizona faults practically parallel to the trend of the contact are known to have had an important influence on the orography of the belt of country in which the contact between Archæan and Paleozoic occurs, but how far if at all the present exposure of Archæan is due to the erosion of Pale-

* Bull. U. S. Geog. and Geol. Surveys, vol. ii, p. 351.

ozoic strata has not been determined. The similarity and continuity of the two areas, however, give color to the supposition that in Arizona also the present contact substantially coincides with the line of post-Carboniferous uplift.

The mining districts on the western edge of the Paleozoic near and north of Owens Lake are at distances from one another too great to justify their being regarded as constituting a belt if taken by themselves, but when their occurrence is considered in reference to the Paleozoic area it seems difficult to avoid the conclusion that they really form a continuation of the Arizona belt.

A conclusion of economical importance evidently follows immediately from the facts set forth, quite independently of any theory whatever. It appears that a great majority of all the profitable ore deposits west of the crest of the Wahsatch occur in belts a few miles in width which follow the western edges of distinct geological areas, the Cretaceous in Utah, the Paleozoic, and usually the Carboniferous, in Nevada and Arizona, the Jura-Trias in Eastern California and the Cretaceous and Tertiary in Western California. Analogy therefore points to the neighborhood of the portions of these contacts still remaining unexplored as the most promising for future discoveries. Geologically it seems impossible to avoid the conclusion that the profound disturbance of the rocks in the neighborhood of these contacts was a condition of the deposition of the ores, thus establishing for these cases an intimate connection between the great events of geological history of the genesis of ore deposits. The upheavals, however, cannot in general have been the immediate causes of the formation of ores, for some of the most important bodies occur associated with later volcanic rocks in such a manner as to prove them far more recent than the upheavals. This again indicates that the fractures which attended some of these great orographical changes have continued through whole epochs unrepaired, as indeed is known in part from other evidence.

San Francisco, office of the U. S. Geol. Survey, May, 1884.

ART. XXIX.—*Notice of the remarkable Marine Fauna occupying the outer banks off the Southern Coast of New England. No.* 9; by A. E. VERRILL. *Brief Contributions to Zoology from the Museum of Yale College.* No. LV.

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Work of the Steamer Albatross in 1883.

DURING the summer of 1883, the new U. S. Fish Commission steamer, Albatross,* Lieut. Z. L. Tanner, commander, continued the work of dredging in the region of the Gulf Stream, along our coast, from off Cape Hatteras to Nova Scotia.† She is, in construction, well adapted to do deep-sea work, and fully equipped with improved apparatus, and therefore was able to carry the dredgings much farther out to sea than the Fish Hawk had been able to, in previous years. The stations most distant from the coast were more than a third of the way to the Bermudas. The greatest depth successfully dredged was in 2949 fathoms, at station 2099, N. lat. 37° 12′ 20″, W. long. 69° 39′, August 2. Besides this, there were four successful hauls in 2033 to 2369 fathoms, and 27 between 1000 and 2000 fathoms. Between 500 and 1000 fathoms there were 19 hauls, and in less than 500 fathoms, 63, making a total of 116 stations. At nearly all these localities a large trawl was used, and in many cases large quantities of specimens were obtained, even at great depths. The bottom temperatures between 1000 and 2000 fathoms were usually between 37° F. and 39° F., and rarely, 40° (in one case, at station 2050, 49·5° was recorded in 1050 fathoms, and at station 2052, on the same day, 45°, in 1098 fathoms, but these cases may have been due to some accidental cause, for at the same date other trials, at similar depths, gave 39°; I have therefore omitted these two temperatures, in the table). Besides the ordinary temperature observations,

* Descriptions of this steamer and of her equipment, as well as of some of her trips, have already been published in Science. An account of the Crustacea was published in the July number of this Journal by Professor S. I. Smith, who has also published a detailed account of that group, with figures, in the Fish Commission Annual Report, part x. The writer has published a paper on the Mollusca, with five plates, in the Trans. Conn. Acad., vol. vi. Some of the new fishes have been described by Messrs. Gill and Ryder.

† The naturalists associated with the writer, in this work, in 1883, were Professor S. I. Smith, Mr. Sanderson Smith, Professor L. A. Lee, Mr. Richard Rathbun, Mr. J. H. Emerton (also as artist), Mr. B. F. Koons, Professor Edwin Linton, Mr. H. L. Bruner, Mr. J. E. Benedict (naturalist attached to the steamer), Mr. R. S. Tarr, W. E. Safford, Ensign U. S. N., and others, more or less. Mr. Peter Parker, Mr. John A. Ryder, Dr. Theodore Gill, and R. H. Miner, Ensign U. S. N., worked on the fishes. The parties who went out dredging, on the steamer, varied from time to time. Usually only three or four naturalists, besides Mr. Benedict, could be properly accommodated on board. I took no part in this portion of the work, in 1883, not going out on the steamer at all.

numerous serial temperatures, not given here, were also taken, and the specific gravity of samples of water, from various depths, was determined. The bottom at all the stations below 1000 fathoms was mainly composed of "Globigerina ooze," usually having the consistency of fine sticky mud, commonly of a dull olive-green or bluish color.* When washed through a very fine sieve a variable, but often large, proportion remains on the sieve, composed chiefly of the shells of *Globigerina* and other foraminifera, of many kinds, but mostly minute. These are usually mixed with a considerable amount of very fine grains of siliceous sand,† among which are some grains of magnetite and garnet. Green grains, apparently of glauconite, are also common.

The deepest localities were all rich in animal life, of many kinds. A considerable number of interesting fishes were obtained, many of them new to our fauna. Some of these are new genera and species of great interest.

Very interesting additions to our collections were made in nearly every class of marine invertebrates, including many undescribed species and genera, some of which are of great morphological importance, while many of the described species were previously known only from distant regions, on the European side of the Atlantic, in the arctic or antarctic regions, off the coast of South America, in the West Indies, or even in the Indian or Pacific Oceans. Thus our knowledge of the distribution of the deep-sea forms, both geographically and in depth, has been greatly increased. Some of these deep-sea species were first described as fossils from the European tertiaries. Moreover a considerable number of our shallow-water species have been found to have a much greater range in depth than was anticipated, many of them going down below 500 fathoms, while some even go below 1000 fathoms.

On the first trip of the Albatross from Wood's Holl, which was made July 16th to 19th, four successful hauls were made with a large trawl, in 1346 to 1735 fathoms, on the 17th and 18th of July, two each day, besides the soundings and tempera-

* In recording the character of the bottom, on the vessels, the character of the mud is usually judged of mainly by its appearance to the naked eye, and sometimes by the sense of touch. The finer varieties of "globigerina ooze" are not distinguishable by these tests from "fine blue mud," or "sticky mud," or "fine gray mud." Nor are the coarser foraminifera always distinguished from "sand" or sandy mud." Thus the official records of this and other similar explorations do not always agree with the determinations of the naturalists who subsequently examine the samples of bottom. In many cases, however, such corrections are eventually made. Very perfect samples of bottom-mud are often enclosed in the hollow, bulbous bases of large *Actiniæ*. Such samples, not having been washed free of their finer portions, might often serve to supplement or correct the official records made from the samples brought up in sounding.

† The *sand* probably floats out on the surface to these distant localities from the *shore beaches*, as explained in my former papers in this Journal.

Partial List of Stations occupied by the Albatross in 1883.

Most of those in less than 500 fathoms are omitted.

Stat.	Locality.		Fath.	Bottom.	Date.	Temp. F.		Hour.
						Bottom.	Surface.	
	N. Lat.	W. Long.						
2034	39° 27′ 10″;	69° 56′ 20″	1346	glb. O.	July 17			
2035	39 26 12;	70 02 37	1362	" "	17			
2036	38 52 40;	69 24 40	1735	" "	18	38°	76°	4.30 A. M.
2037	38 53 00;	69 23 30	1731	" "	18	38	76	1.22 P. M.
2038	38 30 30;	69 08 25	2033	" "	26		76	2.32 P. M.
2039	38 19 26;	68 20 20	2369	" "	28		81	noon.
2040	38 35 13;	68 16 00	2226	" "	29		76	4.20 A. M.
2041	39 22 50;	68 25 00	1608	" "	30	38	72	3.50 A. M.
2042	39 33 00;	68 26 45	1555	" "	30	38·5	71	10.32 A. M.
2043	39 49 00;	68 28 30	1467	" "	30	38·5	72	5.07 P. M.
2044	40 00 30;	68 37 20	1067	" "	31	39	72	5.25 A. M.
2045	40 04 20;	68 43 50	373	bu. M., fine Sh.	31	40	72	10 A. M.
2046	40 02 49;	68 49 00	407	bu. M.	31	40	72	noon.
2047	40 02 30;	68 49 40	389	" "	31	52	72	2.15 P. M.
2048	40 02 00;	68 50 30	547	crs. S., M. & G.	31		72	3.56 P. M.
2049	39 43 40;	69 20 00	1025	bu. glb. M.	Aug. 1	39	71	3.35 A. M.
2050	39 43 50;	69 21 20	1050	glb. O.	1		72	9.15 A. M.
2051	39 41 00;	69 20 20	1106	" "	1	39	72	2.34 P. M.
2052	39 40 05;	69 21 25	1098	" "	1		73	6.16 P. M.
2072	41 53 00;	65 35 00	858	gy. M.	Sept. 2	39	56	6.15 A. M.
2073	41 54 15;	65 39 00	586.5	gy. S.	2	40	58	10.41 A. M.
2074	41 43 00;	65 21 50	1309	fine glb. M.	3	40	69	6.42 A. M.
2075	41 40 30;	65 35 00	855	" " "	3	39	58	3.41 P. M.
2076	41 13 00;	66 00 50	906	bu. glb. M.	4		69	3.20 A. M.
2077	41 09 40;	66 02 00	1255	" " "	4	39	68	8 A. M.
2083	40 26 40;	67 05 15	959	gy. M.	5	40	72	4.30 A. M.
2084	40 16 50;	67 05 15	1290	bu. M., S. F.	5	40	78·5	9.09 A. M.
2093	39 42 50;	71 01 20	1000	F. S. M.	21	39	69	1.12 P. M.
2094	39 44 30;	71 04 00	1022	" " "	21	38·5	68	5.07 P. M.
2095	39 29 00;	70 58 40	1342	glb. O.	30		69·5	9.02 A. M.
2096	39 22 20;	70 52 20	1451	" "	30	37·5	69	2.07 P. M.
	Off Chesapeake Bay.							
2097	37 56 20;	70 57 30	1917	" "	Oct. 1		72·5	5.30 A. M.
2098	37 40 30;	70 37 30	2221	" "	1		72·5	1.08 P. M.
2099	37 12 20;	69 39 00	2949	" "	2		82	5.30 A. M.
	Off Delaware Bay.							
2100	39 22 00;	68 34 30	1628	" "	3	37·5	69	11.05 A. M.
2101	39 18 30;	68 24 00	1686	" "	3	37	67	4.31 P. M.
2102	38 44 00;	72 38 00	1209	" "	Nov. 5	39	62·5	
2103	38 47 20;	72 37 00	1091	" "	5	39	62	
2104	38 48 00;	72 40 30	991	bu. glb. M.	5	45·5	63	
	Off Chesapeake Bay.							
2105	37 50 00;	73 03 50	1395	glb. O.	6	41	63	
2106	37 41 20;	73 03 20	1497	" "	6	41·5	63	

ture determinations, including series of temperatures at various distances from the surface. On this trip about 105 species of Invertebrates were obtained, not including the Foraminifera and other minute forms. There were among these 14 species of Anthozoa; 2 of Hydroids; 22 of Echinoderms; 38 of Mollusca; 15 of Crustacea; 1 of Pycnogonida; 10 of Annelida; 1 of Bryozoa; 2 of Sponges.

The Echinoderms were among the most abundant and interesting of the deep-sea animals. About sixty species were dredged by the Albatross, many of which are new to our coast, though previously dredged on the European side, or in the Caribbean Sea and still more distant regions. Others are undescribed forms. Among the Holothurians were two gigantic species, belonging to a peculiar deep-sea family of which many species were brought to light by the Challenger expedition. These occurred in large numbers at several stations, mostly between 1000 and 1500 fathoms, in some cases more than a barrelful of one of them coming up in a single haul. The largest and most singular one is a new species of *Benthodytes* (*B. gigantea* V.)* which is a very large, oblong, massive species, flat below and convex above, sometimes 18 inches long and 5 or 6 broad, having a gelatinous, translucent appearance, but with a firm cartilaginous texture when fresh. The cartilage-like walls of the body are very thick, often an inch or more, and the visceral cavity is very small in proportion. Owing to the dense and impervious, cartilage-like tissues, this species is very difficult to preserve in alcohol, the interior decaying before the fluid can penetrate the tissues, even when the visceral cavity is cut open.

The second species is also a new form, *Euphronides cornuta*

* *Benthodytes gigantea* V. Body massive, usually elliptical or oblong, broadly rounded at the ends, strongly convex on the upper surface. The whole dorsal surface is smooth and lubricous but covered with numerous, minute, soft papillæ, both above and below. On the upper surface two alternating rows of rather small, tapered ambulacral papillæ run from one end to the other, on each side, about midway between the center and margin, but these usually stand so nearly in line as to appear like a single row, consisting of about eleven papillæ. The margin is crenulated, each crenulation is surmounted by a small, tapering papilla, or modified sucker. The mouth is situated on the under side, a short distance from the anterior margin. The tentacles are twenty, short and thick, terminated by a group of small conical papillæ. The cloacal opening is situated on the upper surface, close to the posterior margin. Two rows of small suckers occupy the median ambulacrum on the posterior half of the ventral surface; the two rows are a short distance apart and often lie in a more or less sunken groove; there are about twelve suckers in each row, the anterior ones smaller and farther apart. The color is translucent, pale flesh-color, or purplish white, reticulated with whitish lines or wrinkles, and sometimes irregularly mottled above with dark purple or dull orange, deeper toward the margin. Ventral suckers and tentacles dark purplish brown. Length of the largest specimens, about 18 inches, breadth, 6. Ordinary specimens, in alcohol, are from 250 to 300^{mm} long by 75 to 100^{mm} broad, and 50 to 70^{mm} thick.

V.,* related to *E. depressa* of the Challenger expedition. It has two pairs of large, elevated, teat-like anterior tubercles, to which character the name refers. In form it is not unlike *B. gigantea*, but it is smaller, narrower, less massive, and has a much thinner, reddish brown integument, without the cartilaginous character of the latter.

The starfishes were very numerous in the deep dredgings and are represented by many interesting species. The most abundant starfish was a fine, new, orange-red species of *Zoroaster*, of large size, with slender spinose arms, (*Z. Diomedeæ* V.)† About 200 specimens of this occurred at station 2035. It was taken at many stations, in 1000 to 1600 fathoms. The most common genus, as usual in very deep water, was *Archaster*, of which numerous species occurred. Many of these are very large

* *Euphronides cornuta* Verrill, sp. nov. A very large species, oblong in outline, with both ends rounded, the upper surface strongly convex, the lower flat, in transverse section nearly semicircular. On the upper surface, a little in advance of the middle, are two very large, divergent, blunt tubercles, often more than an inch in length, usually swollen at base, with a narrow central tube. A little in front of these is a similar but smaller pair, usually about half as large, the size and shape varying according to the state of contraction. In front of the second pair there are usually about four pairs of much smaller tubular papillæ, the anterior ones smallest. At about the posterior fourth there is a very large, double median tubercle, swollen at base, rounded or emarginate at the summit, and terminated by a pair of tubular verrucæ. An orange-brown longitudinal band runs along just outside the bases of all these tubercles, on each side, and branches go from these to the central tube of each tubercle. The margin is thin, with small scollops all around, between all these there is a small prominent papilla or modified sucker. The mouth is situated on the under surface, at about the anterior sixth; tentacles eighteen, short, blunt, thick, covered with small papillæ. Cloacal opening situated on the under side, near the posterior end. A median ambulacrum, visible through the skin, runs between the two openings, with two alternating rows of small suckers near together. The general color is dull flesh-color, or pale brownish, usually with fine specks of orange-brown. The dorsal and submarginal ambulacral bands and their branches running to the suckers and tubercles are deep purplish brown, fading to orange at the margin; the bands are bordered on each side by white or pinkish. Tentacles, buccal and cloacal regions dark purplish brown. The whole lower surface is often purplish brown, reticulated with darker lines or wrinkles. When distended the skin is more or less translucent, with a somewhat gelatinous appearance, but in contraction it becomes more opaque and darker colored. The whole surface is roughened with minute dermal plates. Length of an ordinary specimen, 300^{mm}; breadth, 70^{mm}; height, about 60^{mm}.

† *Zoroaster Diomedeæ* V., sp. nov. Arms five, long, slender, tapered, angular above, with three or five rows of acute spines; one median dorsal, and one lateral, or in large specimens two, on each side; these spines arise from rows of prominent close plates; the surface between is covered with small sharp spines; two or more rows of large single pores between the dorsal and lateral rows of plates; three to five rows of smaller lateral plates along the sides of the rays, with rows of pores between; each plate bears a single long, slender spine and several much smaller ones. Four rows of suckers on the basal part of arms. Adambulacral plates alternately unequal; the larger ones bear each a transverse row of four or five slender, sharp spines; the inner spine projects inward between the suckers and bears 3 to 12 large, elongated, tapering pedicellariæ. Similar large pedicellariæ are scattered over the back and sides. Madreporic plate very small. Color in life orange or orange-red. Greater radius of a large one, 150^{mm}; lesser radius, 13^{mm}.

and handsome, and are generally orange or orange-red in color. Several are unlike those species from less than 500 fathoms, taken by the Fish Hawk. A large, rather smooth *Archaster*, with a very large madreporic plate (*A. grandis* V.,)* and a remarkably spinose species of a related genus (*Benthopecten spinosus* V.),† were associated with the *Zoroaster* at station 2035 and elsewhere, generally between 1000 and 2000 fathoms.

The Anthozoa were abundant, both in individuals and species, in most of the dredgings below 1000 fathoms. About 40 species were taken, altogether, belonging to all the principal groups. Several were undescribed, while others are new additions to our fauna, though previously obtained elsewhere by the Blake or Challenger. It was also a source of satisfaction to us that we rediscovered, in larger numbers, the few remaining species that the Blake and Challenger had obtained off our coast, but which we had not previously dredged.

Among the Pennatulacea there are some highly interesting forms. The remarkable deep-sea genus, *Umbellula*, originally discovered off the coast of Greenland, but hitherto unknown off the eastern coast of North America, though recently dredged

* *Archaster grandis* V., sp. nov. Five long, slender, tapering arms; disk small pentagonal, with concave sides. Dorsal surface unusually smooth, covered with small paxillæ, having minute spinules; among these are scattered three-bladed pedicellariæ. Madreporic plate very large, convex, covered with stellate groups of small spinules, larger than the paxillæ. Lateral plates squarish, the upper and lower about equal and opposite, each bearing a single moderately large, tapering spine, but in the interbrachial spaces the lower ones often bear two or three: surface of plates covered with small spinules. The ventral plates, which cover a triangular space, bear small divergent spinules, often with a larger central one. Adambulacral plates bear a curved, longitudinal group of 8 to 10 close spines, the central ones longest. Oral plates prominent, spinulose, with an oval naked space in the middle between them, and margined by many short oral spinules. Larger radius, 110^{mm}; lesser, 17^{mm}; diameter of madreporic plate, 10^{mm}.

† *Benthopecten spinosus* V., sp. nov. Rays five, long, rather slender, except at base flat, and gradually tapered to long narrow tips. Disk not very large and like the arms covered with a smooth skin and covered plates, each with a slender central spine; these become larger toward the center, where there is a group of about 20 long, erect, tapering, sharp spines. A circle of 4 to 6 papillæ, placed singly, surrounds each plate. There are neither paxillæ nor granules. Upper marginal plates rather small, elevated in the middle, more than 40 on each side; each bears several minute spines and a single central, large and long, tapered, sharp spine, the basal ones and those bordering the disk becoming larger and exceeding those of the center of the disk. Lower marginal plates bear each a vertical row of about 3 spines, the upper one much the longest, and along the middle of the arm as long as those of the upper plates, but becoming much smaller at the base of the arm and along the disk, where the latter increase in size. The ventral plates form small triangular areas; they are rounded, raised in the middle, and each bears one or two small acute spines, but no granules. The adambulacral plates project strongly inward, and each bears an inner convex row of 4 or 5 small, slender spines and a transverse outer row of much larger ones, of which one or two inner are much the largest. Suckers large, tapered, pinched up at tip. Larger radius, 150^{mm}; smaller, 22^{mm}. Still larger examples were taken. Station 2035, in 1362 fathoms, etc.

Benthopecten, gen. nov., resembles *Archaster*, but differs in having no paxillæ; the dermal plates usually bearing a single spine, sometimes two or three.

in most other parts of the deep oceans by the Challenger and other expeditions, is represented by several large and fine specimens belonging to two species, one of which is probably *U. Guntheri* Kölliker, taken by the Challenger in the eastern Atlantic, while the other, *U. Bairdii* V.,* seems to be new. In this genus there is a handsome cluster of large flower-like polyps at the summit of a tall slender stem.

Another new species, which was taken in 1362 to 2369 fathoms, belongs to the genus *Kophobelemnon* (*K. tenue* V.)† This genus we had not previously dredged, though a single specimen of a different species (*K. scabrum* V.) had been obtained by the Blake expedition in 1880. This was also taken by the Albatross in 789 fathoms. A very elegant species, apparently new, belonging to the genus *Scleroptilum* K., hitherto known only from off Japan, occurred in large numbers at several stations, in 1467 to 2369 fathoms. This I have called *S. elegans*.‡ It nearly always had a new species of *Astronyx* (*A. tenuispina* V.)§ clinging closely to it, which, like its host, was bright orange.

* *Umbellula Bairdii*, sp. nov. Stalk very tall and slender; axis quadrangular, with concave sides; basal bulb long, narrow; rachis short, swollen. Polyps eight or nine, bilaterally arranged, large, long, very smooth, with long, slender, regularly pinnate tentacles. Zoöids simple, small, numerous on all sides of the rachis, running up on the sides in lanceolate groups between the polyps; a few extend down in a row on each side of the upper part of the stem. Color, deep orange-brown. Height, 400 to 500^{mm}; diameter of stem, in middle, 3 to 4^{mm}. Stations 2036 to 2038, in 1731 to 2033 fathoms.

† *Kophobelemnon tenue* V., sp. nov. Tall and slender, with a long thin stalk and a long rachis, only a little thicker. Polyps large, short, alternate, in a regular row on each side, well-separated. Zoöids small, not crowded, forming a band on each side of the back, leaving the middle naked, and running up in lateral groups between the polyps; a few are scattered on the ventral side. Spicula small and abundant. Color of stem and rachis light yellow; of zoöids orange-brown. Height, 4 to 6 inches. Stations 2035, 2038, 2039, in 1362 to 2369 fathoms.

‡ *Scleroptilum gracile* V., sp. nov. Slender, with the polyps occupying more than half the length. The polyps are large, arising from large and somewhat conical incurved calicles, which are swollen at base and covered all over with an abundance of small slender spicula. The calicles are frequently opposite, or nearly so, but more generally alternate; the lower ones become gradually smaller and more scattered, and very small ones extend for some distance down the stem, which is very slender, with a long narrow bulb. The zoöids are small but prominent and scattered along the ventral side, mostly in groups of 3 to 6 alternating with the pairs of polyps, but often they occur also between their bases. Color orange in life, yellowish white in alcohol. Height, 185^{mm}; of polypiferous portion, 110^{mm}; breadth across contracted polyps, 3 to 4^{mm}; diameter of rachis and stem, 1 to 1·5^{mm}; length of calicles, about 3^{mm}; diameter at base, 2^{mm}. The swollen bases of the calicles are often filled with eggs. Stations 2036, 2038, 2039, 2041–3, in 1467 to 2309 fathoms.

§ *Astronyx tenuispina* V., sp. nov. A species with a pentagonal disk and five long slender arms, much resembling *A. Loveni*, but easily distinguished by having, instead of hooks, three slender, divergent arm-spines, of which the lower is much the longest and largest, with the distal end enlarged and roughened by little spinules. The genital openings are situated as in *A. Loveni*, but not quite so near together. Mouth-papillæ small and short; the jaws end in a single short spine-like papilla. Color orange, in life; white in alcohol.

Perhaps this ought to constitute a new genus, on account of the spines.

Among the other interesting Pennatulacea is *Anthoptilum Murrayi* K., which was dredged several times in 640 to 1362 fathoms. It was first taken off Nova Scotia by the Challenger. It was associated, in some cases, with the much larger *A. grandiflorum* V., first described by me in this Journal from numerous fine specimens obtained by the Gloucester fishermen on the banks off Nova Scotia, but afterwards described by Köl-liker as *A. Thomsoni* from specimens dredged by the Challenger off Buenos Aryes, in 600 fathoms.

The Gorgonacea are represented by several fine species, some of them conspicuous on account of their large size and bright colors. The bush-like *Acanella Normani*, as usual in deep water, occurred abundantly. *Lepidisis caryophyllia* V., which grows in the form of tall, slender, unbranched stems, often two or three feet high, with long, hollow, calcareous joints, alternating with short horny ones, and with very long spiny calicles, occurred alive several times, in 1098 to 1735 fathoms, and dead in many localities, where its joints are very abundant on the bottom and afford solid support for the attachment of other species of Anthozoa, etc. Fine living specimens of *Acanthogorgia armata* V. were taken in 407 to 640 fathoms, and a number of specimens of *Primnoa lepadifera* were dredged on the outside of Brown's Bank, off southern Nova Scotia, in 101 to 131 fathoms. A very elegant plumose coral, the *Dasygorgia Agassizii* V., which has a slender, iridescent, calcareous axis, with the main branches spirally arranged and the large polyps obliquely seated, was dredged in 1346 to 1362 fathoms. It belongs to a peculiar deep-sea family, Chrysogorgidæ,* recently established by me for this and several related genera, nearly all of them elegant in form and colors. A new genus, belonging to this family, was dredged in 858 to 1735 fathoms (*Lepidogorgia gracilis*).†

* Blake Anthozoa, Bulletin Mus. Comp. Zool., xi, p. 21, 1883.

† *Lepidogorgia*, gen. nov. Axis simple, iridescent, with calcareous, ramose roots; polyp-cells large, oblique, covered with fusiform spicula; cœnenchyma thin, covered with small oblong scales.

Lepidogorgia gracilis V. Axis tall, slender, terete, tapering to a flexible tip; roots large, thick, irregularly but finely arborescently branched, the branchlets white, round, tapering and coral-like. Polyp-calicles large, prominent, distant, obliquely seated, often thicker than the stalk, covered with large spicula, longitudinally placed. Spicula of the cœnenchyma small, flat, oblong, or constricted in the middle, with rounded ends. Color, when fresh, orange or salmon. Height, often 900^{mm} or more; diameter of axis, at base, 1–1·5^{mm}; of polyps, 1^{mm}; distance between polyps, 5 to 10^{mm}. Station 2072, off George's Bank, in 858 fathoms; stations 2036 and 2037, in 1735 and 1731 fathoms. A large lot from station 2037.

ART. XXX.—*Geology of the Blue Ridge near Balcony Falls, Virginia; a modified view;* by JOHN L. CAMPBELL.

IN this Journal, vol. xviii, Dec. 1879, will be found a paper on the geological structure of the Blue Ridge, at Balcony Falls, Rockbridge County, Virginia, illustrated by a map and geological section. As to their main features the facts there presented are correct; and the descriptions given have been corroborated by subsequent observations—at least so far as relates to the Cambrian formations that rest against the western flank of the igneous and metamorphic core of the Blue Ridge at this point.

But, as regards the stratified beds flanking the main ridge on its eastern slope, subsequent observations have modified the views expressed on page 439 of the paper referred to above, where the following paragraph occurs:—

"The bedded rocks (1, *a b*,) that rest upon the syenite are very much metamorphosed, are gneissoid in character, and dip toward the southeast. [Correct so far]. Then follows a bed of forty or fifty feet of conglomerate and quartzite, bearing some resemblance to the conglomerate sandstones on the opposite side of the ridge, but so different in composition, texture, position and thickness as to preclude the idea that they have any historical connection. Over this again we find another bed of slate. These beds all dip toward the southeast while their upper margins reach beyond the underlying syenite and granulite, and with their edges support the lowest beds of Primordial rocks, where both extend high up on the ridges, beyond the limits of the igneous beds."

It was a mistake, as will appear farther on, to class these as *Archæan beds*. But some apology may be found for my mistake in the fact that Prof. W. B. Rogers, in his notes on the geology of Virginia, as found in Macfarlane's Geological Railway Guide, speaks of the rocks between Lynchburg and Balcony Falls as all belonging to the Laurentian and Huronian (his A and B) formations. I had not, however, seen that note of his before the paper above quoted was published; so that I am entirely responsible for my own partially erroneous conclusion.

After my paper on this subject had been published, suspicions arose in my mind that the conglomerate and slate beds along the eastern flank of the main Blue Ridge might be of Cambrian age, and so modified by the metamorphic agencies as to have their characteristic features obscure; but engagements elsewhere diverted my attention from them until recently.

My assistant, Mr. Harry D. Campbell, and myself were requested, a few months ago, to make a professional examination of a belt of slate lying along the southeast base of the Blue Ridge in the western corner of Amherst county. In this belt the "Snowdon Slate Quarries" have been opened, and on tracing its strike toward the southwest it was found to cross the James River a little beyond the limit of the map and section referred to above.

While examining the geological relations of the belt of slate, my assistant discovered that one of the beds of sandstone underlying the slate, abounded, at some points, in *Scolithus borings*. Examinations extended lower down the River on the Amherst side disclosed several alternations of sandstones, conglomerates and slates, all dipping toward the southeast, but with decreasing steepness until, as they approach the old ferry where the Richmond & Alleghany Railway crosses the river, they become approximately horizontal. Then changing their dip toward the northwest with increasing steepness, a similar succession of beds of sandstones and slates was found, with the lowest resting upon Archæan rocks, such as constitute the core of the main ridge.

Thus we found what is manifestly a somewhat shallow syncline, about three miles wide, running parallel with the general range of mountains, and occupied by successive beds of sandstones, conglomerates and slates, bearing a decided resemblance to the lower members of the Cambrian beds, on the N.W. side of the main mountain, though very considerably modified by metamorphism; so much so that I, as well as others formerly regarded this syncline as of Huronian age, and I pointed it out as such to Prof. C. H. Hitchcock some two or three years ago as we passed it on the Richmond & Alleghany train; but the subsequent discovery of a bed of sandstone *in situ*, containing characteristic *Scolithus borings*, settles the question that the beds in question are of Cambrian age.

In confirmation of our conclusion, Mr. H. D. Campbell has traced the Scolithus bed, with its accompanying conglomerate for at least five miles on each side of the river For some distance from the margin of the river, on the Amherst side, the higher members of the series appear to have been removed by denuding agencies; but on the S.W. or Belford side, he found the "Balcony Rock"—the lowest bed of quartzite—well exposed; then, in the next higher sandstone bed, he found abundant traces of Scolithus, corresponding in position and range with what had been found on the other side. Some of the higher peaks a little remote from the river, he found capped with the main Scolithus bed, the upper or typical Potsdam sandstone of this region. We may conclude therefore, that

this portion of the Blue Ridge has been formerly spanned by a grand arch, or series of arches, of Cambrian age, upturned perhaps at the time of their upheaval—the broken fragments of which have been carried away, and only the abutments left to tell the story of a great catastrophe.

Washington and Lee University, August, 1884.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS.

1. *Photographing Colored Objects in their Natural Shades.*—Professor H. W. VOGEL has laid before the Physical Society of Berlin the results of his long continued researches on this subject. It is well known that unnatural pictures of colored objects are generally obtained by photography, the darkest shades of blue appearing white, the brightest tints of yellow and red as black, and so on. Professor Vogel has endeavored to render his plates less sensitive to less refrangible rays by alloying the silver coating with some substance capable of absorbing these rays. He has obtained in eosine, and more especially in its various derivatives, coloring substances which possess merely a broad absorption band in the yellow, and which give the desired results. By mixing these bodies in the right proportion on the dry gelatine plates, the yellow of the colored objects appeared quite clear on the photograph; but the blue was brighter. Thereupon Professor Vogel inserted between the object and the camera a yellow glass, which partially absorbed the blue rays, leaving the yellow unimpaired. In this way photographs can be obtained, in which the green and yellow and to some extent the red portions of colored objects present the same vivid effects as the originals.—*Nature*, June 19, 1884, p. 188. J. T.

2. *Measurement of Magnetic Forces by means of Hydrostatic Pressure.*—G. QUINCKE has shown that electric force produces a pressure upon insulating liquids placed in an electric field. The effect is the production of a tension parallel to the lines of electric force, and of a nearly equal pressure at right angles to the lines of electric force, which are proportional to the square of the electric force at the point of the electric field where the experiment is made, and also proportional to the dielectric constant of the fluid under consideration. He has now extended this investigation to magnetic and diamagnetic fluids placed in magnetic fields. From analogy we should have in this case $p = \frac{K}{8\pi} H_1^2$ (Maxwell's Electricity and Magnetism, 2d ed., ii, p. 257, § 642), in which H_1 is the magnetic force at the point in question in the magnetic field, and K is what may be called by analogy the diamagnetic constant. The value of this for air is 1. "If the magnetic fluid is bounded by air, the magnetic difference of pressure in the fluid and in the

air may be expressed by a hydrostatic pressure, and thus $K_1 - 1$ can be measured." The magnetic fields employed by Quincke varied in strength from 300 c. g. s. to 12,000 c. g. s. The liquids were placed in the magnetic field in a square trough of nickel-plated brass plate, with vertical sides of plate glass and a round iron base, which was placed upon the lower horizontal pole surface of a Ruhmkorff electro-magnet, the upper pole plate being brought within a distance of $1{\cdot}7^{mm}$ to $3{\cdot}3^{mm}$. A central opening in the upper pole surface was connected by means of a thin brass tube with a sulphide of carbon manometer and a long India-rubber tube provided with a tap (Wied. Ann., xix, 1883, p. 719). It was found that under the influence of magnetic force the hydrostatic pressure at the bounding surface of air and the magnetic or diamagnetic fluids, parallel and at right angles to the lines of magnetic force, undergoes the same increase or decrease; and that the resulting pressure is of the same magnitude parallel and at right angles to the lines of magnetic force. The magnetic increase in height was in some cases more than 32^{mm} with magnetic fluids and $-0{\cdot}7^{mm}$ with diamagnetic fluids. Quincke gives a full table of the values of the magnetic effect upon different liquids, and shows that the method adopted might be practically employed to measure the intensity of a magnetic field. He also shows that the superficial tension of a magnetic fluid in contact with air is not sensibly altered in a strong magnetic field. The quantity which G. Wiedemann terms atomic magnetism (Pogg. Ann., cxxvi, 1865, p. 23) can also be calculated from Quincke's results.—*Berliner Sitzungsberichte*, Jan. 17, 1884; *Phil. Mag.*, June, 1884, p. 447–459. J. T.

3. *Production of very low Temperatures.*—M. L. Cailletet has discovered that formene or Marsh gas can be employed to liquefy oxygen without the employment of mechanical means for lowering the temperature of boiling of the cooling liquid. Formene is easily compressed and cooled in ethylene boiling under atmospheric pressure. In the state of a liquid it is extremely mobile, and in passing to the gaseous state lowers the temperature sufficiently to immediately liquefy oxygen. This method makes liquefication of oxygen a very simple process.—*Comptes Rendus*, p. 1565, No. 26, June 30, 1884. J. T.

4. *Use of the Silver Voltameter for the measurement of the Electric current.*—In a recent communication to the Physical Society, Lord Rayleigh stated that he considered the deposition of silver by a current of electricity the best method of measuring the current in absolute measure. One Ampère deposits 4 grams of silver per hour; a sufficient amount can therefore be obtained in fifteen minutes which can be weighed with accuracy. With very weak currents there is difficulty in weighing the deposits; and with very strong currents the deposits are apt to be irregular. Pure nitrate or pure chlorate of silver give the best results. The cathode of Lord Rayleigh's apparatus is a platinum bowl, the anode a silver sheet wrapped with clean filter paper sealed over

it to keep any loose silver from dropping on the cathode. The anode is immersed in the solution of silver salt; and at the end of several hours a measurement of the weight of silver deposited is made by weighing the bowl cathode in a chemical balance. This process is found preferable to weighing the loss of weight of the anode.—*Nature*, July 17, 1884, p. 283. J. T.

5. *Change of temperature due to Magnetization and Demagnetization.*—M. Bachmetieff has made at the University of Zurich an extensive series of experiments on this subject, the first part of which is now published in the Journal of the Russian Chemical Society (vol. xvi, fasc. 3). He arrives at the conclusion that magnetism irrespective of currents of induction produces changes of temperature in magnetized bodies and that this "magnetic heat" is equal to the product of the magnetic moment by the magnetizing force multiplied by a constant. Its cause must be sought for in purely mechanical forces; and it depends upon the speed of rotation of the molecular magnets.—*Nature*, July 3, 1884, p. 223. J. T.

6. *Radiation of a Swan incandescent lamp at different temperatures.*—H. Schneebeli employed in his investigation the method of Svanberg and Langley. The resistance of the lamp while cold was 80 ohms. The following tables exhibit the results:

No. 1.			No. 2.		
Current strength.	Entire radiation.	Light radiation in candles.	Current strength.	Entire radiation.	Light radiation in candles.
44·5	87·0	0·12	48·0	102·0	0·3
48·8	97·0	0·25	76·2	254·0	5·5
57·0	158·	0·70	94·2	392·0	24·0
67·0	195·	1·85			
75·0	250·	5·4			
88·2	348·	17·5			

For each current strength, J, there exists the following relation, $J^2W=CS$, in which W is the resistance of the carbon at the designated temperature, S the energy emitted by the lamp, and C is a constant. If W, the resistance of the lamp, is constant between 900° and 1500° temperature, one has $\frac{J^2}{S}$ = constant. This is expressed by the following values:

J.	44·5	48·8	57	67·0	75·0	88·2	48·0	76	94·2
$\frac{J^2}{S}$	22·8	24·5	20·7	23·0	22·5	22·4	22·6	22·8	22·6

The resistance of carbon between red heat and white heat appears to be independent of temperature. The absorption coefficient of the glass globes of the lamp appears also to be independent of the temperature between these limits.—*Ann. der Physik und Chemie*, No. 7, 1884, pp. 430, 438. J. T.

7. *The Electrical Exhibition at Philadelphia.*—The Electrical Exhibition which the Franklin Institute is making extensive preparations to hold in Philadelphia, during the months of September and October of the present year, promises to be a large and

representative one. Exhibitions devoted exclusively to electricity and its applications have been held with most pronounced success in Paris, Munich and Vienna, at each of which America participated largely, and carried off her full share of honors. The subject is one which occupies a large share of the public attention; and an exhibition in this country, which shall creditably represent the great progress which the science has made since the period of the Centennial, and which shall especially show how largely the world is indebted to American discovery and invention for the advances that have been made, will not only possess an unusual attractiveness, but if proper use is made of the opportunity, will prove most beneficial as a popular educator. For this reason it is most fitting, that the first electrical exhibition held in America should be undertaken by a scientific body of such assured standing and unquestioned respectability as the Franklin Institute of the State of Pennsylvania.

The buildings, in which the exhibition will be held, occupy several blocks, and are situated at 32d and Lancaster avenue, in West Philadelphia, within a convenient distance from the heart of the city, and readily accessible from all directions by the steam and street railways. The exhibition is announced to open on September 2d, and to close on October 11th. The system of classification which has been approved by the committee having the work in charge is very elaborate, and, in its general features, original. The exhibits will be grouped under the following seven sections: Section I, Production of Electricity; Section II, Electric Conductors; Section III, Measurements; Section IV, Applications of Electricity; Section V, Terrestrial Physics; Section VI, Historical Apparatus; Section VII, Educational and Bibliographical. Each of these sections is subdivided into numerous classes.

The committee charged with the duty of preparing a schedule of the tests to be conducted, of the apparatus and machinery, has prepared and published a code in accordance with which the work shall be carried out, and has invited a large number of eminent specialists in electricity and mechanics to coöperate in the work. These tests will include the steam boilers and engines, as well as the special electrical apparatus and machinery.

The project of the exhibition is international in character, and a considerable number of foreign exhibits will make their appearance. The foreign exhibitors will be principally in the classes devoted to educational apparatus, and instruments of precision for electrical measurements. This country will be represented by all the leading companies, manufacturers and inventors engaged in the field of electricity and magnetism covering of course very completely the subjects of the electric telegraph; electric lighting by the several systems (arc and incandescence); the telephone; microphone; electric signaling systems for railways; electric clocks, time telegraphs and other electric devices for recording or registering at a distance; electricity as applied to mining, naval and military engineering, to light-houses, to musi-

cal instruments, to the transmission of power, to galvanoplasty and to metallurgy; and a multitude of other applications.

Many of these exhibits which are being prepared by the leading companies engaged in electric lighting will be very large, and all will be in the highest degree instructive. The historical portion of the exhibition will be full; and the collection of the literature of electricity and magnetism will be an attractive feature to the student and professional man.

The utility of the exhibition will also be enhanced by a series of popular lectures and by a thorough system of labelling, for the benefit of the many to whom the exhibit would be otherwise unintelligible.

The meeting of the American Association for the Advancement of Science will take place in Philadelphia while the exhibition is in progress, and many members of the British Association, which meets this year in Montreal, have signified their intention of being present. To this gathering will be added the American Institute of Mining Engineers, the American Institute of Electrical Engineers, and other bodies less prominent.

Anticipating this gathering of scientific forces, and with the view of utilizing them for the advancement of science, the Franklin Institute appealed to Congress to authorize the holding of a "National Conference of Electricians" to convene in Philadelphia during the period of the exhibition, to discuss matters of interest affecting the present state and future progress of the science, and to make public the results of their deliberations. This project was favorably acted on by Congress and approved by the President, who has caused to be announced through the Secretary of State, the appointment of the following gentlemen as members of a commission "which may in the name of the United States conduct a National Conference of Electricians . . . and have the direction of the scope and character of the work of the conference," viz: Professors H. A. Rowland, M. B. Snyder, J. Willard Gibbs, John Trowbridge, C. A. Young, G. F. Brackett, Dr. W. H. Wahl, Professors Simon Newcomb, G. F. Barker, E. J. Houston, R. A. Fisk, Francis C. Van Dyck. The first meeting of this commission was held on Thursday, August 7th, in Philadelphia, at the Hall of the Franklin Institute, under the chairmanship of Professor Rowland. From the scientific eminence of this commission, the results of the Conference promise to be of the first importance.

As another evidence of the interest which the forthcoming electrical exhibition has attracted, it is worthy of note that the Royal Society of London has deemed it of sufficient importance to appoint four of its members: Dr. John Hopkinson, Mr. W. H. Preece, Lord Rayleigh, and Sir William Thomson, to act as its representatives.

No other exhibition undertaken by private enterprise, has ever promised to exercise the proper function of an exhibition so truly as an educational institution, and it is to be hoped, that by the intelligent use of its great opportunity, the Franklin Institute

will prove itself equal to the task it has set itself to perform, and add fresh laurels to those it has earned in the past. W.

8. *Études pratique sur les Marées fluviales et notamment sur le Mascaret, application aux Travaux de la Partie maritime des Fleuves,* par M. COMOY. 389 pp., large 8vo, with an atlas of ten plates. Paris: 1881 (Gauthier Villars; New York, F. W. Christem).—This work by M. Comoy is a valuable contribution to a difficult and complex, and at the same time, highly interesting subject. The author approaches it from the practical side, through the results of numerous personal observations and published records rather than by means of mathematical analysis. The opening chapters contain a simple discussion of waves of translation and oscillation, after which tidal waves in general are considered, and then the more difficult subject of tidal waves in rivers—all in a clear and satisfactory manner. The chief interest of the volume centers in the discussion of the Mascaret, a phenomenon to the study of which the author has devoted much time and labor. The mascaret is the name given in some parts of France, as on the rivers Garonne and Dordogne, to the peculiar wave which advances up the river something like a wall at the head of the tide; it is a similar phenomenon to the Bore on the Ganges, the Prororoca in the Amazon and the Barre on the Seine. It is a capricious phenomenon on some rivers of a country, varying much in its action, sometimes almost disappearing and again regaining its original intensity. The author discusses this portion of his subject with much thoroughness, going over the views of others as to the cause of the phenomenon and then developing his own on the same subject. The author shows the effect upon the mascaret of the varying conditions, such as depth of river-bed, width of channel, etc.; he also discusses the influence upon the fluvial tides of public works undertaken in the rivers to facilitate navigation. In the final chapter he gives a detailed study of the principal rivers of France in their relation to the tides.

II. GEOLOGY AND MINERALOGY.

1. *Preliminary Paper on the Terminal Moraine of the Second Glacial Epoch;* by T. C. CHAMBERLIN. 110 pp. 4to, with maps and plates. From the 3d Ann. Rep. of the Director of the U. S. Geological Survey, 1881–82.—This important paper by Professor Chamberlin is briefly noticed at page 68 of the last volume of this Journal. It commences with a statement of general principles with regard to the drift, and a description of the materials, their kinds, arrangement and distribution, and then gives details with regard to the structure, forms and geographical arrangement of the "Terminal Moraine of the Second Glacial Epoch."

The relation as to position between this second terminal moraine and the southern limit of the "drift-bearing area," or the extreme limit of proper glacial transportation, is well presented on the

map making plate XXVIII. The two limits are not separated along the islands south of New England. Westward, the extreme limit has the course laid down by Messrs. Cook and Smock for the part of it in New Jersey, Lewis, in Pennsylvania, and Wright, in Ohio and Indiana; and farther west it crosses the Mississippi at St. Louis, reaches its southernmost point, on the parallel of 38°, along the Osage River, and then runs northwestward and northward, 30 to 100 miles west of the Missouri.

The moraine belts referred to the "second glacial epoch," in New York and west of it, lie 50 to 500 miles north of the southern drift limit. They extend southward in "loops" that show the outline of distinct glacier movements, or at least so, where the morainic feature is well defined and it is supported in evidence by divergent glacial striation and transportation. The important observations on these moraine loops by Professor Chamberlin have thrown much light on the glacial phenomena of North America. A brief statement of his general results is given by him in a paper in this Journal, published in August, 1882, and part of the details earlier in volume ii of the Reports of the Wisconsin Geological Survey. In the present report, excellent maps and full descriptions are presented of the wonderful loops made out by him to correspond to a Green Bay and Michigan Lake glacier; and also of other morainic loops, such as the Grand Traverse, the Saginaw, the Maumee or Western Erie, the Scioto and others.

Professor Chamberlin has extended his careful observations eastward over New York and the marginal regions of the glacier farther east. He describes loops in western New York; and east of these, a Mohawk Valley moraine (making the movement of the ice west of Herkimer to have been westward, while east of there, eastward); and a Hudson River moraine; and he suggests, from the south-by-west and south-southwest movement in the Connecticut Valley, the idea of a "tendency to minor lobation" in that valley. But he remarks as regards eastern New York that he is not ready for any positive opinion.

The writer has given his views as to the Connecticut Valley ice recently in this Journal, and will only add here that he has not yet observed in New England any facts that can be referred to a second glacial epoch, or any morainic deposits away from the highest mountains that are not a consequence of or dependent upon bottom movements in the glacier mass determined largely by the forms of the underlying land.

The map of the courses of glacial movements in the Hudson River valley indicate, if sustained by further observation, that the ice moved from Albany, in that valley, over the Mt. Washington summit in southwestern Massachusetts, where the ice (since the summit is 2,624 feet high and is glacier scratched) had probably a height of at least 3,000 feet; and this implies, consequently, that the height of the ice at Albany, forty miles north-northwest, was greater than this—probably 3,500 feet. But, as the striæ north

and northeast of Albany have a south-southwest direction a higher ice-summit existed to the north-northeast toward or within the area of Vermont. It seems probable that all the phenomena can be accounted for, by the view, diverging a little from Professor Chamberlin's, that New England with nearly all New York was under one broad lobe of the glacier having its greatest height along or in the vicinity of the Taconic, Green and Adirondack Mountains, and other lands in that direction farther north; that from this axial portion the movements were eastward, southeastward and southwestward—the Catskills, overtopping, as Smock has shown to be probable, the surface of the southwestwardly-moving ice. This conclusion appears to flow from Professor Chamberlin's map, plate XXXIII, and it accords, as far as New England goes, with the writer's published views. The query remains, as to how far the morainic loops of the eastern half of New York may be accounted for on the view of one glacial epoch, and one glacier subdivided *at bottom* by the valley depressions of the underlying land.

Prof. Chamberlin, in speaking of the *Long Island moraine deposits*, expresses his dissent from Mr. Upham's view that the two lines, the northern and southern, (which are distinct along only a portion of the island, and are but five to fifteen miles apart,) correspond to the moraines of the first and second glacial epochs, urging as an objection the great improbability that the two would be so nearly parallel; and he suggests that they are more likely to be parallel morainic lines of one and the same epoch. The writer here adds that he has satisfied himself that the double line of elevation in Long Island was a configuration of the surface that preceded the era of glacial depositions. The underlying sands, gravels and clays, up to a level usually of eighty feet and often greater, are Tertiary (where not Cretaceous), and the subdivisions into high or low regions appear to have existed in these older deposits of Long Island before glacial deposition took place. Over Long Island all the high lands are covered with bowlders, many of large size; but over the lower lands, that at the head of Peconic Bay which separates the higher lands into a northern and southern range, and those of the great bay-like valleys of the north coast, as well as that of the long gradual slope of the south coast from 80 feet to sea-level, bowlders are almost uniformly absent (the cases of slips down the bluff fronts excepted). So complete an absence occurring in the midst of the deposits of a terminal morame is a striking fact. The writer has found no way to solve the puzzle except by supposing that these low parts were occupied by water when the drift deposition took place; that the stones fell into the water and were afterward covered up by stratified deposits of sand and gravel, for this is the constitution of the deposits of these lower lands of the island. The white and yellow Tertiary clays, white sands and gravel are beautifully displayed at Stuart's beds near the middle of the island west of Hicksville about 80 feet above the sea. Further, I was led to

conclude that the Peconic Bay depression cut through the Tertiary, and the deep depressions occupied by the broad and many-armed bays of the north coasts (which are of wonderful extent for a land of so little height having no rocks in sight) were part of the results of under-glacier streams of the Glacial era, before glacial deposition had made much progress; that these were thus made deep, and afterward became submerged in the opening of the Champlain period, the era of subsidence, melting and deposition.

The writer may here state also that in 1873, in his paper on the Glacial and Champlain eras in New England (this Journal, v, 210), he makes the suggestion that the coarse drift of Long Island was probable evidence of *a terminal moraine deposit from the melting southern edge of the retreating glacier.* It is still true that nobody knows whether the glacier may not have extended miles outside of Long Island when at its farthest limit, a view the same paper favors. The principal objection to it comes from the now known fact, that the terminal moraine of New Jersey is nearly in a line with the southern coast of Long Island.

J. D. D.

2. *Pennsylvania Geological Survey. Report Z: on the Terminal Moraine in Pennsylvania and Western New York;* by H. C. Lewis. 300 pp. 8vo, with maps, photographic views and sections. Harrisburg, 1884.—This report has special interest because of the length, form and features of the "terminal moraine" which crosses the State of Pennsylvania and also because of the thoroughness of the survey. In much of the work Professor Lewis was associated with Professor G. F. Wright and the report is partly a result of their combined labors. The glacial phenomena are described in detail for each of the glaciated counties. The results prove, as has already been announced in this Journal, that the glaciated part of Pennsylvania occupies two great areas, a large northeastern, and a very small northwestern, a fact due to the crossing by the terminal moraine of the north boundary of the State into Cattaraugus County, New York. The terminal moraine is described as a line of drift hills made "mainly of glaciated material," but as "often inconspicuous;" as, in many parts a succession of conical hummocks with kettle-holes, the latter sometimes the sites of ponds; as frequently a limit between a more northern region of many lakes, and a more southern of no lakes, the lakes owing their existence mostly to the damming effect of the drift deposits. The mean width is about a mile. Large stones and bowlders are abundant—while there are comparatively few north of the line. As the character and height of the country passed over by the moraine line is of much interest, we cite here the general account of its course, from page 47 to 49 of the report.

"Beginning in Northampton county, a mile below Belvidere, it appears through the stratified drift as low gravel hills. These, winding up over the slate hills to the west, are soon developed

into an accumulation of typical *till*, holding kettle-holes and filled with bowlders. Bending in a great curve first westward and then northward it reaches the base of the Kittatinny Mountain three miles east of the Wind Gap. Ascending to the top of the Kittatinny Mountain (1600 feet above tide) the moraine crosses it, being well shown upon the very summit, and, entering Monroe County, crosses the great valley between the Kittatinny and the Pocono, enclosing in its course several moraine lakes. Having crossed this valley and reached the base of the Pocono escarpment it swings sharply back and around Pocono Knob. Immediately afterward it ascends the steep face of the mountain to the wide plateau on top, 2100 feet above the sea. Crossing the center of Kidder township, Carbon County, it reaches the gorge of the Lehigh River about ten miles north of Mauch Chunk, which it crosses at Hickory run. Without swerving from its general northwestern course it crosses the Hell-Kitchen Mountain, Cunningham valley, and Nescopec Mountain, in Luzerne County, and descends to the valley of the east branch of the Susquehanna River, which it crosses at Beach Haven. Here heaps of drift have been washed down the river into terraces. In Columbia County, after following awhile the base of Lee's Mountain it ascends to the summit (1350 feet); crosses the high red shale valley, and the crest of Huntingdon Mountain; and then descends the north slope of that mountain to the broad undulating valley of Fishing creek. Taking a northerly course it follows up the *east* bank of Fishing creek to the North or Allegheny Mountain.

The summit of the Alleghenies in Sullivan County is covered with glacial striæ and other marks of glaciation. In Lycoming County the moraine passes westward along the base of the mountain, crossing Muncy and Loyalsock creeks, and then, near the village of Loyalsock, turns at right angles and ascends to the highlands. Having reached the summit of the Alleghenies (over 2000 feet above the sea) it crosses the picturesque cañon of Lycoming creek, and passing west through a wild, wooded region nearly as far as Pine creek, it takes a nearly straight northwestward course through the southwest corner of Tioga County and the northwest part of Potter. On the high ground of Potter County it crosses a great continental water-shed from which the waters flow into the gulf of Mexico, Lake Ontario and Chesapeake Bay. Here the moraine is finely shown at an elevation higher than anywhere else in the United States, 2580 feet above tide.

The line of the moraine now enters the State of New York in the southwest corner of Allegheny County. Running still northwest and entering Cattaraugus County, it twice crosses the winding course of the Allegheny river, east and west of Olean; then, trending to a point five miles north of Salamanca, in latitude 42° 15′, it suffers a remarkable change of direction. Turning at right angles to its former course it passes southwest through the southeast corner of Chautauqua County, and keeping approximately parallel to the Allegheny river, reënters Pennsylvania in Pine Grove township, Warren County.

It crosses the Conewango River seven miles north of Warren, forming immense accumulations in the valley of the river. Then, trending west in Warren County (still at a general elevation of nearly 2000 feet above the sea) the moraine crosses one gorge after another, and forms a line separating not only the glaciated from the non-glaciated regions, but also the cultivated from the uncultivated and densely wooded regions." After crossing Crawford, Venango and Butler Counties, the southeast corner of Lawrence and the northwest of Beaver County, it "crosses the Ohio State line in the middle of Darlington township, thirteen miles north of the Ohio River, in latitude 40° 50′. The moraine thus leaves Pennsylvania at *precisely the latitude* at which it entered the State."

"The total length of the moraine is about 400 miles. It crosses the Delaware at an elevation of 250, the Allegheny at an elevation of 1425, and the Beaver at 800 feet above the sea (225 feet above Lake Erie); but upon the high lands of Potter County it rests on ground nearly 2600 feet above tide and its surface must have been about 3000 feet."

The thickness of the till is stated to vary from a mere sprinkling of bowlders to 100 feet or more; it is even 200 feet deep in many parts of northwestern Pennsylvania, while in eastern, it is generally thin.

Mr. Lewis gives for the usual limit of transport of bowlders in Pennsylvania 10 to 20 miles. The thickness of the ice at the terminal edge in *some valleys* is estimated at about 700 feet, and five miles back 1000 feet; but where the edge of the ice was diagonal to the trend of the mountains, the thickness was in some cases greater, as near Stroudsburg, 8 miles back from the limit, it must have been 2000 feet. Scratches and a large bowlder on Penobscot Knob, 2200 feet, make the probable height of the ice over the knob 2500 feet; and this was only 8 miles north of the "terminal moraine." In his introductory letter accompanying Mr. Lewis's report, Professor Lesley makes the height over the Kittatinny Mountain, in eastern Pennsylvania, at *the least* 600 feet, and 1800 feet over Cherry Valley to the west. Examples are mentioned of large bowlders resting on elevations more than 1000 feet above their source; two such on the Kittatinny Mountain, one of them twenty feet in length; a class of facts known to be well illustrated by bowlders on Katahdin in Maine, and others carried from Canaan into Goshen, Connecticut; showing that the bottom ice, as it was pressed forward in consequence of the upper surface of the glacier rising to a higher level to the northward, moved up the encountered slope carrying its gathered load of stones.

The direction of the glacial striæ over the eastern ice-region of Pennsylvania is described as southwesterly, while the same over the northwestern, is southeasterly.

The most remarkable feature brought out by the investigations is the bending of the moraine northwestward in crossing the

state of Pennsylvania, and then its bending around in Cattaraugus County, New York, for another southward extension. Consequently the glacier between Cape Cod and the Boston headlands, on the east and Cattaraugus County on the west, had an arched front, the chord of which arc along latitude 42° 15′ was nearly 500 miles long, and the versed sine, about 120 miles. It was the southern front of the portion of the great ice mass which covered New England and New York, and the regions to the northeastward, from whose higher portions the movement of the ice was southeastward and southwestward. Professor Lewis's report is fully illustrated by maps and views.

J. D. D.

3. *Geological Survey of Pennsylvania.*—Besides the volume by Professor H. C. Lewis, above noticed, the survey has issued recently also the following reports:

Report D3, Vol. II, Part I: The Geology of the South Mountain belt of Berks County; by E. V. D'Invilliers, with maps and plates, and an atlas, 1883.

Atlas to D4, Vol. I and II.

Report H7: a revision of the Bituminous Coal Measures of Clearfield County; by H. M. Chance. 198 pp. 8vo, with maps and sections.

Atlas of the Western Middle Anthracite field to illustrate Report AA, Part I, Chas. A. Ashburner, Geologist-in-charge.

4. *Palæontology of the State of New York*, vol. v, Part I, *Lamellibranchiata.* I, Text and Plates, containing *descriptions and figures of the Monomyaria of the Upper Helderberg, Hamilton and Chemung Groups;* by James Hall, State Geologist. 268 pp. Albany, 1884.—The volume of text now issued was preceded a year and a half since by a volume of 80 plates, which is briefly noticed on page 391 of volume xxv of this Journal (May, 1883). To these plates 12 more are now added, Nos. 81 to 92. The species described are those belonging to the families *Pectenidæ*, *Pterinidæ*, *Aviculidæ*, *Ambonychidæ* and *Mytilidæ*. The Lamellibranchiates discovered in the New York Paleozoic now number about 600, and 500 are from the groups above the Oriskany sandstone. Professor Hall recognizes among the inequivalve species, under Pectenidæ, 5 genera, the Pterinidæ, 3 genera, the Aviculidæ, 7 genera; and among the equivalve species, under Ambonychidæ, 1 genus (Byssopteria), and under the Mytilidæ, 4 genera; and fifteen of the new genera are instituted by him. The volume opens with a synopsis of the classification and brief descriptions of the genera. The plates are most excellent, and the work is a rich contribution to Zoological science as well as American Paleontology.

5. *Octahedrite as an alteration-product of Titanite.*—Mr. J. S. Diller has made some interesting observations on the occurrence of octahedrite (anatase) as a product of the alteration of titanite in the biotite-amphibole-granite of the Troas. After describing the unaltered rock in regard to the more conspicuous char-

acters, the author goes on to mention some highly altered varieties observed near the village of Tavacly. In this altered granite the feldspar has been changed either into a mass of minute highly refracting scales, or to a fine granular mass with weak aggregate-polarization; the mica and hornblende have completely disappeared and in their place there is a greenish fibrous substance with carbonates and numerous crystals of a yellow mineral not found in the fresh rock; the zircon is unchanged, but the titanite has also completely disappeared. It is to the last change that the greatest interest is attached. The mineral which has taken the place of the titanite has a wine-yellow to honey-yellow color and shows under the microscope quadratic and rhombic sections; the former isotropic, the latter strongly doubly-refracting with the extinction-directions parallel to the diagonals. The crystalline system is thus found to be tetragonal, the form that of a square pyramid. Cleavage parallel to the base and to the pyramid was also observed. By use of the Sonstadt solution the specific gravity was found to be between 3·6 and 4·5. Also under the microscope the terminal pyramidal angle was determined from the sections to be 98° 24′, and the basal edge of the rhombic sections 136° 16′. These angles, as also the characters previously given, agree with those of octahedrite; moreover, the presence of titanium was also proved by chemical means, thus leaving no doubt as to the identity of the mineral. These observations are of interest in view of the often observed alteration of titanite and titanic iron, and the author regards that the production of octahedrite is a common result, though frequently overlooked on account of the minuteness of the crystals.—*Jahrb. Min.*, 1883.

6. *Chlorophane from Amelia County, Virginia.*—Mr. George F. Kunz has recently presented to the New York Academy of Sciences a note on some specimens of the variety of fluorite called chlorophane, from Amelia County, Va.,—the locality which has furnished the remarkable specimens of microlite and monazite. The occurrence of fluorite at the locality has been described by Fontaine (this Journal, xxv, 333), who also noted the brilliancy of the phosphorescent light which it gives out at a low temperature. Mr. Kunz states that, while handling in the dark a few specimens of this mineral, phosphorescence resulted by the slightest attrition of the specimens, either one with another, or with a nail or hard substance. An examination of ordinary fluorite from several localities, did not show this property of phosphorescence by attrition, while it appeared conspicuously in specimens of chlorophane from Amelia County, Va., and North Chatham, N. H.; it consequently appears to be a distinguishing character of chlorophane. Pallas mentions a specimen from Siberia, of a pale violet color, which gave out a white light merely by the heat of the hand; by the heat of boiling water, it gave a green light, and when on a live coal, a bright emerald green light that might be discerned from a distance. In a dark room at a temperature of about 80° Fahr. the Amelia County mineral shows a white lum-

inous light, which by the warmth of the hand is intensified; when placed in boiling water it becomes green, and on a heated iron plate it becomes an intense emerald green. Most of the material is more or less flawed, so as to render it very friable to the touch; this variety seems to phosphoresce at even a lower temperature than the more compact form, and is either a light green or a yellowish green.

7. *Brief notices of some recently described Minerals.*—MANGANOSTIBIITE, AIMATOLITE, and AIMAFIBRITE are three new minerals from the Moss mines in Nordmark, Sweden, recently described by L. I. Igelström (Geol. För. Förh., vii, 210 et seq., 1884).—*Manganostibiite* occurs with other manganesian minerals in the crystalline limestone enclosed in the ordinary granulite of Sweden. It appears as small black grains, probably belonging to the orthorhombic system. It is compact, with difficult cleavage and greasy luster; the powder is brownish. It is infusible before the blowpipe, and perfectly soluble in hydrochloric acid. An analysis afforded:—

Sb_2O_5	As_2O_5	MnO	FeO	CaO	MgO
24·09	7·44	55·77	5·00	4·62	3·00=99·92

for which the formula $5MnO, (Sb, As)_2O_5$ is calculated.

Aimatolite (from *αἱματοειδής*, color of blood—the name should properly be hæmatolite or hematolite). Also occurs in cavities in the limestones in small crystals of rhombohedral form with basal cleavage. The color is transparent and of a fine red like that of precious garnet, the powder is brick red. An analysis gave:

As_2O_5	MnO	FeO	MgO	CaO	H_2O
25·70	34·55	13·05	8·10	2·52	16·08=100·00

for which the formula $2(Mn_3As_2O_8)+8MnH_2O_2+6H_2O$ is calculated.

Aimafibrite (hemafibrite) occurs in a mixture of magnetite and serpentine. It forms spheres a centimeter in diameter, which are radiated and fibrous in structure, the form belonging, according to observations by Bertrand, to the orthorhombic system. The color is blood-red, resembling the mineral just described. An analysis gave:—

As_2O_5	MnO	FeO	MgO	CaO	H_2O
29·94	46·98	4·65	2·00	1·50	14·93=100

for which the formula $2Mn_3As_2O_8+7MnH_2O_2+6H_2O$ is calculated. Hematolite and hemafibrite are very near each other in composition, and they are also near to the allaktite of A. Sjögren, recently described from the same locality (this Journal, xxvii, 494).

Utahite is a supposed new iron sulphate from Eureka Hill, Juab County, Utah, described by Arzruni. It forms a yellow-brown crystalline crust on quartzite. The individual crystals are very minute; their form is a hexagonal prism with rhombohedral planes on the alternate angles. The cleavage is prismatic; the luster silky. An analysis by M. Damour gave:—

SO_3	As_2O_5	Fe_2O_3	H_2O
28·45	3·19	58·82	9·35=99·81

for which the formula $3Fe_2O_3$, $3SO_3+4H_2O$ is calculated, which separates it more or less clearly from the other known iron sulphates.—*Bull. Soc. Min.*, vii, 126, 1884.

Goyazite.—M. Damour has given the name goyazite to a phosphate of alumina and lime which he has identified among same minerals received from the diamond sands of the province of Minas Geraës in Brazil. It is found in small rounded grains, one to five millimeters in diameter. They are more or less transparent, of a yellow white color; they show one cleavage. The hardness is that of apatite and the specific gravity 3·26. An analysis gave:—

P_2O_5	Al_2O_3	CaO	H_2O
14·87	50·66	17·33	16·67=99·53

For this the formula P_2O_5, $5Al_2O_3$, $3CaO+9H_2O$ is given, which requires: P_2O_5 14·38, Al_2O_3 52·19, CaO 17·02, H_2O 16·41=100. —*Bull. Soc. Min.*, vii, 204, 1884.

III. Botany and Zoology.

1. Gray's *Synoptical Flora of North America*, Part II, is at length issued. It comprises the Gamopetalous orders from *Caprifoliaceæ* to *Compositæ* inclusive, thus concluding the Gamopetalous orders. An enumeration, which precedes the full index, indicates that of the *Caprifoliaceæ* there are 8 genera and 47 species; of *Rubiaceæ* 26 genera and 86 species; of *Valerianaceæ* 2 genera and 22 species; of *Dipsacaceæ* 1 genus and 2 species (naturalized); of *Compositæ* 237 genera and 1610 species. The part runs to 474 pages, or 72 more than in Part I. Yet it is less thick than its predecessor, the paper, though excellent, being thinner. It bears the imprint of Ivison, Blakeman & Taylor, New York, Wm. Wesley and Trübner & Co., London, and O. Weigel, Leipsic. It will also be sent by mail, postpaid to any part of the United States and Canada, by addressing The Curator of the Herbarium, Cambridge, and enclosing the price ($5), as a convenience to distant botanists.

2. *The Orchids of New England: a popular Monograph;* by Henry Baldwin. New York: John Wiley & Sons. 1884. pp. 158, 8vo.—Botanists will not find in this volume a monograph of the Orchideæ of New England in their sense of the term. It is, as the title declares, "a popular monograph," a collection of what has been ascertained and written about our Orchids, by one who has paid particular attention to them, and who has recorded many observations of his own upon their habits, their appearance, and their structure, and some upon their insect-fertilization. But upon this subject, in which very much remains to be done, the author has generally preferred to quote. Having entered upon this interesting line of observation, Mr. Baldwin ought not to stop upon the threshold. There is perhaps not one of our

species that has been sufficiently studied in this regard; and the study may be expected to render a second edition more scientifically and independently valuable, without any sacrifice of popularity. The figures might be better, and they would have been made more telling by neat magnified views of the parts. A. G.

3. *The Student's Flora of the British Islands;* by Sir Joseph D. Hooker. Third Edition. London: Macmillan & Co., 1884. —This third edition gives evidence that the great popularity of this well-planned and well-executed Flora continues undiminished. It is not increased in size; the changes, though not inconsiderable in the treatment of certain groups and species, are not striking; but the revision has been evidently been critical. *Subspecies* are more largely adopted: varieties accordingly play a diminished part,—a fair compromise between the schools of narrow and wide limitation of species, and perhaps a necessity in the long-worked floras of Europe. "Characters concerned in the process of fertilization" are also introduced into this edition. A good beginning, but, to serve their purpose in a popular flora, more particularity would seem to be needed, and the dichogamy which is attended by change in position (as in cases like *Scrophularia*, *Epilobium angustifolium*, &c.) might well be indicated. The different modifications of a peculiar structure in *Campanula* (of which only a rarer form is mentioned in the generic character) might well come into the description of the species or sections. The use of the term *homogamous* for flowers which are neither proterandrous nor proterogynous (i. e. not dichogamous) is unfortunate (though it was introduced by C. C. Sprengel), being a term of common use for quite another thing, from Lessing down to the new Genera Plantarum. If A. W. Bennett's term of *synacmy* does not well take an English adjective form, why not use the ready-made and obviously apt word synchronous? A. G.

4. *Hypopitys or Hypopithys?*—As the work above noticed and the recent Genera Plantarum adopt the latter orthography, and DeCandolle the former, it is worth while to determine, if possible, which best conforms to the rules of nomenclature. There is no question that the former is orthographically correct and that the latter is a falsified form. The older botanists on the Continent, not pronouncing the *h* in *th*, seem to have thrust it into various words according to their fancy, probably thinking that a silent letter could do no harm. If we turn to Linnæus, to the classical Endlicher, and to Bentham and Hooker, we should infer that they had done so in the present instance. But we should be wrong. Although Linnæus (who did not adopt the genus) cites "Orobanche quæ Hypopithys dici potest" from Bauhin, and "O. Hypopithys lutea" from Mentzel, in both these authors the name is *Hypopitys*. So far as we know, the adulteration of the word was the work of Linnæus, in the Species Plantarum, and it can be shown to be accidental. For the genus was founded and illustrated by Dillenius, in 1719, as *Hypopitys* (not *Hypopithys*, as Endlicher cites it); and, in referring it to *Monotropa*, Linnæus in

every edition of his Genera Plantarum, writes "*Hypopitys*, Dill.," showing that he had no intention of altering the form of the word. We may therefore conclude that Jussieu and DeCandolle and their successors have followed the rule of priority as well as the sense of the word in writing (not *Hypopi'thys*) but *Hypo'pitys.*

A. G.

5. *The structure and function of Lenticels.*—Heinrich Klebahn confirms Stahl's description of the minute anatomy of these organs, and shows by experiments in winter (similar to those by Haberlandt in summer), that they aid in the transfer of gases and aqueous vapor. He further shows that in some shrubs devoid of lenticels, rifts in the bark, connecting with the intercellular spaces, serve the same purpose.—*Ber. d. deutsch. bot. Gesellsch.*, I, ii, 113.

G. L. G.

6. *The microchemical detection of Nitrates and Nitrites in Plants* is possible by the reagent suggested by Wagner, viz: Diphenylamin. Molisch uses a one per cent, or a one pro mille solution in pure sulphuric acid, applying this to dry sections. If either of the salts above named is present, a deep blue coloration appears, which soon changes to brownish yellow. Brucin in about the same strength is nearly as sensitive a test, producing a transient red or reddish-yellow color. Molisch employs this method also for approximate determination of the amounts of the salts present, and finds that the percentage decreases from below upward in the plant.—*Ber. der deutsch. bot. Gesellschaft*, i, 150.

G. L. G.

7. *On the structure and growth of Palms.*—Branner gives in the Proceedings of the American Phil. Soc., April 18, 1884, some interesting results of original and apparently careful studies of the mode of growth of many palms. The essential points of difference between these results and those obtained by other observers relate to the development of the fronds. "The fronds are developed in connection with the *central* bundles in the phyllophore. In regard to the origin of the bundles it is sufficient at present to say that they originate at the apex of the phyllophore, and are developed in it, with it, and as a part of it. Von Mohl and Mirbel maintain that these bundles grow up into the phyllophore; Gaudichaud that they grow downward from it, from the frond bases; Von Martius, that they grow both up and down, while I maintain that they are perfected in all directions at the same time, though the lateral growth continues to a certain extent after the longitudinal growth has ceased, and that they can no more be said to grow upward or downward than it can be said of the bones of the body that they grow outward into the limbs. It is true that the general lengthening of the bundles takes place at the superior end, but there is a growth beside this. At the first appearance of the fronds at the apex of the phyllophore the fibro-vascular bundles are already connected with them, and just as intimately as they are in the perfectly developed frond. The internodes at this point are very short, but the bundles are the

same in number, and have exactly the same connections, directions and relation to each other that they have in later life. But in the perfected frond we find them larger, longer and harder, and in the perfect stem the internodes are longer, the stem and bundles larger, while the whole plant has grown both longitudinally and laterally." G. L. G.

8. *On the Physiological significance of Water-glands and Nectaries;* by WALTER GARDINER. (Proceedings of the Cambridge Philosophical Society, Nov. 12, 1883.)—These Water-glands are peculiar structures, consisting of modified parenchyma-cells, which occur at the end of fibro-vascular bundles, are protected by epidermis, and communicate with the exterior through one or more water-pores. The cells in these glands are rich in protoplasm but are devoid of chlorophyll. Between the ducts of the bundle and the cells of the gland, there are numerous tracheids which form a transition from one to the other form of tissue.

The exudation from the glands consists in most cases of nearly pure water as in Fuchsia, or of saline solutions, as in many of the Saxifragaceæ. The amount of liquid secreted by these glands has been shown by Sachs to be dependent on root-pressure, and to be greatest at the period of minimum transpiration. Substituting the pressure of a column of mercury for root-pressure, Moll confirmed the results obtained by Sachs. Volkens holds that these glands constitute a mechanism of the nature of a safety valve to relieve too great hydrostatic pressure in the plant, and that where such glands with pores are not present, the office is performed by slightly modified stomata. At this point Gardiner takes up the subject, substantially confirming the results of the others, and adding notes relative to the differences between these glands as they occur in monocotyledons and dicotyledons. No gradual stages between them can be detected as yet, by comparative study, and this gives some support to the view that these groups of plants have been regarded as early branches from a common ancestor, and that there is not a gradual ascent from one group to the other. Gardiner further calls attention to the fact, but lays no special stress upon it, that whereas dicotyledons are typical land-plants, the reverse is true of monocotyledons. Even the apparent exceptions to this broad statement can be partly explained away, such orders as Gramineæ, Aroideæ, and Orchidaceæ, needing for their growth a plentiful supply of moisture. Now we have in monocotyledons glands of very simple structure, and here there is little or no resistance put in between the vascular bundle and the free surface. In dicotyledons there are well developed glands in some plants like Callitriche, where we should suppose they cannot possibly be needed. In the glands of dicotyledons there is a very pronounced resistance to the escape of water, and a greater root-pressure is required to bring about exudation. The constituent cells of the plant-tissue are also rendered more turgid in consequence of the increased pressure, and the water is,

so to speak, parted with less freely. In monocotyledons, where the supply of water is fairly constant, these special precautions for economizing the water supply are apparently not taken.

Of nectaries the author takes substantially the view of Sachs, namely, that their activity is dependent upon the activity of the nectar-cells themselves, and is independent of root-pressure. His views conflict with those of Wilson, who attributes the activity to osmosis. Wilson based his conclusions upon the passage of water through the cuticular membrane of leaves when there is placed thereon any substance which can cause osmosis, and the fact that the passage ceases wholly when the substance is withdrawn. But Wilson, at any rate, does not explain the exudation of the nectar required to start the osmotic apparatus of the gland. Moreover, Gardiner objects to the use of a strong solution of sugar by Wilson in his experiments, since nectar in some cases contains only one per cent of sugar. He quotes from Sachs the statement, that the secreting cells "absorb water (containing substances in solution) with great force on one side and exude it on the other," and this solely by their own living activity. The grand period of such activity may be and probably is of short duration, and the further exudation of nectar may be occasioned in the way that Wilson pointed out, namely, by simple osmosis. The paper by Gardiner is accompanied by an excellent set of figures illustrating the structure of the water-gland of *Saxifraga crustata.* G. L. G.

9. *Respiration and Transpiration of Fungi.*—The following conclusions have been reached by Bonnier and Mangin (Annales des Sc. Nat., Mar. 1884), after a series of carefully conducted experiments with apparatus of novel construction. 1. Respiration is increased by elevation of temperature, but there is no optimum. 2. Diffused light retards it. 3. The more refrangible rays of light are most efficient in respiration. 4. The rate increases with increased moisture in the air. The ratio of the amount of carbonic acid evolved to that of the oxygen absorbed varies in different species but is less than unity, but for the same species does not vary with pressure. The relation is constant under all temperatures. As regards transpiration, the following results are recorded: Elevation of temperature augments the rate; and so does diffused light, but increased moisture in the air diminishes the rate. G. L. G.

10. *The Principles of Zoogeography;* by Theodore Gill. 40 pp. 8vo. (Proc. Biolog. Soc. of Washington, 1884.)—In this paper the author considers the subject of geographical distribution both for the land and oceans. Under the latter head Dr. Gill arrives at five *primary* marine regions: the *Arctalian* or Arctic realm, not well defined in southern limit, but having its limiting isocryme coinciding nearly with Cape Cod on the eastern coast of America, and the Straits of Fuca on the western; (2), the *Pararctalian,* or north temperate realm, including the various coast regions between the isocrymes of 44° and 68° F.;

(3), the *Tropicalian*, or Tropical realm, between the isocrymes of 68° F. north and south of the equator, or the Torrid zone or Coral reef seas of Dana. (4.) The *Notalian*, or south temperate realm, and extending, "provisionally," between the southern isocrymes of 68° and 44° F.; and (5) the *Antarcticalian* realm, or Antarctic realm. The above pertain to surface or shallow-water regions. Dr. Gill names the deep-water region, the Bassalian realm, putting the whole in one division, though recent investigations apparently point to at least two.

The author concludes from his survey of the subject, that "freshwater types are the best indicators of the early relations of respective regions;" "the flying, and especially migratory, types are most accordant with the actual relations of land areas;" "temperature is a prime factor, and land a secondary, in the distribution of marine animals." The important factor as regards marine life, light, well set forth by Fuchs, is not recognized.

III. Miscellaneous Scientific Intelligence.

1. *Earthquake in the Middle and Eastern States, August* 10, 1884.—On Sunday, August 10, between seven and eight minutes after 2 P. M. the Middle and Eastern States experienced a decided earthquake shock, more severe and extensive than has occurred in the region for some years.

The area affected extends along the coast from Washington, D. C., and Baltimore, Md., to Portland, Me., and Burlington, Vt., and on the west may be bounded by a nearly straight line from Burlington to Harrisburg, Pa. The intensity was greatest in the vicinity of New York City, which is also about the center of the district affected. It nowhere did any damage beyond breaking crockery and occasionally cracking a house wall. No bodily injury was caused, although it is stated that one or two deaths resulted from the mental effect of fright upon persons in feeble health. The time was very near 14h. 7¼m. Several close time observations have been reported, but it is not yet possible to say whether they indicate a progress of the wave in any particular direction. So far as yet compared, the reports seem to point to the formation of a fault in the geological strata somewhere in the neighborhood of New York Bay, as the probable origin of the vibration.

A fuller notice may be expected in a later issue. C. G. R.

Errata.—Article by Professor Langley, on page 171, line 3 from top, the formula $(Aa^2 = Bb^2) - (Aa^3 - Bb^3)>$, should read $(Aa^2 + Bb^2) - (Aa^3 + Bb^3)>$.

Article by Mr. S. W. Ford, on the age of the rocks in the vicinity of Schodack Landing: on page 206, the last sentence of the note should read: It is possible therefore that the two groups here come into contact, etc.; and on page 270, 12th line from the foot, the name Honstein should be Hauenstein. In Professor Campbell's article, p. 222, 25th line from foot, three should be two.